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On the Formation of Urinary Calculi. Read before the Baltimore Pathological Society, March 2d, 1858, by Prof. CHARLES FRICK, M.D.

I stated to the Society at a meeting held more than a year since, that I had been led for many reasons to doubt the explanation of the causes to which urinary calculi have been attributed. Further investigation into this matter has served only to strengthen this doubt, and I propose this evening, with your permission, to enter into an explanation of the reasons that have led me to the conclusions I shall advance. I would state, moreover, that these ideas, although original, to the best of my knowledge, with myself, are yet not entirely new—a somewhat similar suggestion having been offered by Sherer, some few years since when treating of the subject of the acid and alkaline fermentation of the urine.

Urinary calculus has always proved so serious a disease, that it is not astonishing that for many years various attempts should have been made to find some mode of effecting their solution, and thus avoid the formidable operation of lithotomy. When medical chemistry began to unravel and arrange the various recondite phenomena of the animal body, it seemed a necessary consequence that the urine, the only animal fluid independent of the laws of vitality, should be among the

first to exhibit some practical results. And it is only a few years back, that in the study of the various diatheses—the lithic, the oxalic, and the phosphatic—we were seduced into a fancied explanation of many of the phenomena of both health and disease. It is the custom to underrate those who seized with avidity this so attractive portion of their medical studies, and who were led too far by seemingly practical results. And these labors are considered to be not only without profit, but as indications of unsound reasoning and false pathology. This, I must admit, is in a measure true. However, it is not the fault of the facts if the deductions have been pushed too far, nor has the knowledge thus obtained been by any means without profit. In the history of the world, we are struck with the fact, that the greatest results arrived at in the advancement or civilization of mankind, have rarely been attained by seeking them directly, but have nearly always been brought into view in the search for some other object. Thus it was that the desire for gain, which made men traverse continents and seas, and endeavor to find the shortest passage to the Indies through the Arctic Seas, was the means, though unsuspected and indirectly, of promoting christianity and civilization, and benefiting all mankind. And thus, again, the alchemists, in the search for the philosopher's stone, begat when they little suspected it, a science that was to revolutionize the world. To bring the comparison within narrower limits, and refer to urinary pathology, as it has been termed, who does not see that if it has failed to explain, as definitely as was at first supposed, various specific diseases, of what immense importance it has been in elucidating the phenomena of various healthy and morbid actions not only connected with the urinary apparatus, but with nutrition generally?

But, as I have said, the distinction into the uric, oxalic, and phosphatic diatheses is now no longer tenable; and we are forced to confess, that in classifying and naming diseases in accordance with the nature and quantity of the various substances contained in the urine, we fell into the same error as the old physicians, who classified diseases according to their individual symptoms, instead of grouping them together in natural divisions, founded on distinct processes. While this erroneous view was taken of urinary pathology, it seemed a necessary induction that the formation of calculi should be dependant upon some peculiar salt, whose excess was thus deposited. But as these concretions were found to be composed of various layers, differing in chemical composition, the fact was readily admitted, that each subsequent layer was in great measure produced by the preceding;

and hence the composition of the nucleus became the real subject of practical importance; many pathologists deeming that if the diathesis which produced this nucleus could be altered, the subsequent deposition of stone might be avoided; and consequently, that an individual in whom the microscope revealed a constant deposit of uric acid or oxalate of lime might have the subsequent chances of a calculus removed by changing the diathesis which produced these substances in excess. That such hopes have not been fulfilled, is the common experience of all who have directed their attention to the subject.

The first point, manifestly, that offers itself for consideration, is the chemical nature of calculi. To solve this, I have tabulated the analysis of three different collections—that of Guy's Hospital, the Transylvania University, and the Tennessee collection—numbering in all 600 different specimens, and quite sufficient for the purpose of illustration. Thus in—

	Guy's Hospital.	Transylvania.	Tennessee.	Total.
Uric Acid	250	32	1	283
Urates	19	26	49	94
Triple Phosphate	22	7	108	139
Phos. Lime			2	
Oxalate of Lime	47	7	15	69
Cystin	11	2		13
Foreign matters		4	1	5
Uric Oxide	1			1
	350	78	176	604

The relative proportion of these different ingredients varies, as is seen, between very wide limits, and I cannot but think that the mode of distinguishing uric acid from the urates must have differed in the analyses of these different collections. But that will effect but little the general results I aim to deduce. Suffice it, then, that we find these substances existing in the following proportion:

Urates and Uric Acid	63 per cent.
Phosphates	23 "
Ox. Lime	12 "
Cystin	2 "
Uric Oxide	1-6 of 1 "

I propose to consider in detail the physiology and pathology of these substances, in order to see what is the extent of our knowledge concerning them, and how far we are warranted in considering any excess as constituting a diathesis; what causes induce their deposition, and what part the urinary mucous membrane plays in effecting it.

The first of these, then, is uric acid and its compounds; but as its relations with urea, and its connections with it, are so intimate, I shall be forced to commence with this latter, the most important ingredient in a physiological point of view of the whole urinary secretion.

Urea.—The origin of urea and the sources of its derivation are not yet entirely determined, although numerous observations and experiments have been undertaken with this view, by most of the leading physiologists and chemists of the present day. Liebig, Bischoff, and others on the one side, have arrived at the conclusion that this substance is entirely derived from the metamorphosis of the nitrogenous tissues—while Lehmann, Bidder, Frericks, and Schmidt, admitting this source, maintain that in addition the quantity excreted is most unquestionably influenced by the amount of nitrogenized food. To myself, the facts stated as a result of Lehmann's experiments alone—provided they be reliable, and no one as yet has attempted to impugn them—seem to leave no other alternative than to adopt the explanation given by him. That its source is not entirely derived from the food is proved by the fact, that urea has been found in the urine of an insane man, who had taken no nourishment for fourteen days ; and also in these patients who, laboring under fever or other diseases, have taken no aliment but an oily emulsion, or some emollient decoction. But on the other hand, that albuminous food does exert an influence over its formation, we have only to refer to the experiments of Lehmann and others, who show that its amount in the urine is always increased or diminished in the same individual, as the diet consists at one time of albuminous, and at another of non-nitrogenized food. As to the seat of its formation, numerous observers having detected it in normal blood, we may consider it as now established, that urea is not primarily formed in the kidneys.

In the present state of chemical knowledge, it is impossible to assume what particular tissues undergo the metamorphosis into urea; but knowing, as we do, that the substance is a very general product of the decomposition of nitrogenous matters, both within the animal body and in the chemist's laboratory, we cannot but conclude that it is most probably derived from the disorganization of all into which nitrogen enters as an element. Liebig failed to detect it in the muscular tissue, although he found substances from which urea could be produced artificially, such as creatine and inosic acid. It is therefore most probable, that one or both these substances are decomposed in the blood, by the alkalis and free oxygen, into urea, and other substances destined for excretion.

From what has been said, it follows as a necessary consequence, that the normal amount of urea must vary within very wide limits, dependant upon the two conditions already stated, namely, waste of tissue, and the amount of nitrogenous food. But as a result of numerous experiments, it may be concluded that 525 grains of urea are passed in the 24 hours, by the average of men in the prime of life. Sherer, Rummell, and Bischoff have paid great attention to this point, and from a table made up from their experiments, and detailed in the *British and Foreign Review*, for 1855, we deduce the following interesting facts:

At.....	3 yrs.	4 yrs.	5 yrs.	7 yrs.	18 yrs.
The amount of urea in grains in 24 hours.....	209	240	280	281	562
For every pound of weight there was excreted in grains.....	8.16	9.4	9.4	7.1	5.5

	22 yrs.	31 yrs.	38 yrs.	65 yrs.
The amount excreted in 24 hours	416	605	459	296
For every pound of weight	3.7	4.5	3.6	2.8

Thus showing that the largest proportional amount is in early childhood, and diminishes as we advance in life. This is entirely in accordance with the theory of its derivation, for we know that the metamorphosis of tissue, as well as the digestion of alimentary material, takes place with greater rapidity in the young than in those more advanced in life.

It is a mooted question as yet, as to the influence the free use of water exerts on the metamorphosis of tissue, and the consequent formation of urea. Bischoff's experiments led him to the conclusion that abundant water drinking decidedly increased the excretion of this substance, while Vogel and Schmidt on the contrary affirm, that it is diminished under its use. Some twelve experiments of my own in relation to this matter, fully substantiate the fact as enunciated by Bischoff.

As to the different articles of diet, Bischoff again shows, that fat materially retards the disintegration of the nitrogenous constituents of the body, and therefore diminishes the amount of urea excreted. This result I also had attained, before I was aware of his experiments. I ascertained that, in the urine of a large number of patients to whom cod liver oil was administered as a remedial agent, the urea underwent a sensible diminution; and I had supposed that this might be one of the explanations of the benefit derived from its use, in those suffering from tubercular disease, and those affections characterized by waste of tissue.

Bischoff also has shown that urea is increased under the administration of common salt. A dog was fed for one week on one pound of meat daily, during which time its weight remained constant; it was then fed for twelve days on the same food, with the addition of a daily allowance of 770 grains of common salt. During the first period the daily amount of urea was 362 grains, and during the second, 436 grains.

Böckler, Heller, and Lehmann have all ascertained that the use of tea and coffee diminish the amount of urea excreted. More lately Dr. Hammond has published the results of some experiments upon himself, in corroboration of the same idea. An average of ten analyses for ten successive days are given, during which time no tea nor coffee was taken. The result obtained was 670.6 grains. Compared to this, is the average of ten similar experiments while using daily thirty-two ounces of a strong infusion of black tea. The result is 617.7 grains; and while using the same amount of strong coffee, 639 grains. We are also indebted to the same observer for the results of very accurate observations, showing the influence of increased and diminished mental labor upon this substance, as well as the effects of increased and diminished exercise. The experiments were performed in the same way, and the same number of observations taken as before. The result of increased intellectual labor for fourteen hours daily, gave as an average 748.3 grains. As compared with this, we find he records 586.7 as a result of diminished intellectual labor, the healthy mean being still 670.6.

The results obtained by him under different degrees of exercise, he records as follows:

	Quantity of Urea.	Quantity of Uric Acid.
Moderate exercise	682	13.7
Increased "	865	8.2
No "	487	24.7

The relation of these two substances, urea and uric acid, to one another, here so marked, I shall speak of presently.

The question of most importance to us in the present consideration, is the relation of these two substances. Liebig was the first who advanced the hypothesis that urea was formed by the oxidation of uric acid; and although this has been denied, yet the weight of authority at the present day inclines to this view. There are some facts that are irreconcilable with this theory; for, were it strictly true, an increase of urea should always be in relation with a diminution of uric acid; and moreover, an increased supply of oxygen should produce an increased supply of urea. But Lehmann's experiments do not sub-

stantiate the first, nor can we very well explain the fact that birds, whose respiration and circulation is very rapid, should excrete no urea. On the other hand, Dr. Hammond, quoted above, supports the view of their mutual relation, as has been already shown. This view is also sustained by my own experiments, instituted several years since upon the convicts of the Maryland Penitentiary. I found uric acid in excess in those who were allowed but little exercise, and that in the same individuals the quantity of urea was proportionably diminished; while in those, on the contrary, who, from the nature of their occupations, underwent more muscular exertion in a better atmosphere, there was a larger quantity of urea and a corresponding decrease of uric acid. Still more decided is the illustration offered by Dr. Hammond, as to the influence of oxygen. He confined a black snake in a large jar, and fed it upon flies, grasshoppers, and other insects; at the end of a week he examined the excrement, but found no urea. He then fitted to the jar an air-tight stopper, through which he introduced a large quantity of oxygen gas, daily. At the end of the week, the excrement being again examined, undeniable evidences of the presence of urea were obtained. We therefore cannot but conclude that the probabilities are all in favor of Liebig's theory, although there still remain some facts to be explained before it can be universally recognized.

Not a great deal can be said as to the influence of remedial agents upon the excretion of urea. Those selected for experiment have usually been the alkalis or the alkaline salts, such as tartrate of potash and soda, the phosphate of soda, and liquor potassæ, in small doses. The effect of these has been to increase its amount to a greater or less degree. The most reliable experiments are those made by Dr. Parker, of London. He states that the alkaline salts of potash, and probably also soda, aid the diminution of urea in rheumatism, pneumonia, variola and typhoid fever. He says, moreover, that nitrate of potash seems also to do this in disease, but not necessarily in health.

Little information has as yet been acquired in regard to the changes that urea undergoes in different diseases. But Heller and Vogel have done more than other observers in relation to it. The first, states that the greatest increase was observed in acute meningitis, and that in pneumonia, acute tuberculosis, and rheumatism the urine is very rich in urea during the state of exudation, but diminishes during resorption.

What, then, is the pathological value or therapeutical indication to be derived from an increase or diminution of urea. I cannot think that

its quantity will ever throw any light upon the nature of special diseases, or afford us any indications for treatment. Its presence, as has been shown, being due to metamorphosis of tissue and ingestion of nitrogenous food, any excess must be owing to one or the other of these causes. Consequently, we meet with it whenever waste is going on rapidly, or when an excess of nitrogenous aliment is taken as food; and its deficiency is always due to the reverse of one or the other of these conditions. We find oftentimes, in diseases affecting the structure of the kidney, that there is a great deficiency of urea in the urine. But this is because, although formed, it is not excreted. It still remains in the blood and produces poisoning, probably not as urea, but by being converted into carbonate of ammonia, by decomposition. As to an excess of urea occurring under such circumstances as to produce a distinct disease or diathesis, this I have never seen, and I am confirmed in this opinion by Lehmann stating the same thing. It can under no circumstances evidence more than an effect; and it would seem to be more philosophical as well as scientific, to endeavor to ascertain this cause, than to be led into the error of mistaking a symptom for a disease.

After what has been stated, we are ready to admit that *uric acid* is a substance one degree higher in the scale of descending metamorphosis of matter than urea. We cannot assert of it more than of urea, the special substances from which it is first produced, or the locality in which it is formed. But in all probability the tissues in disintegration pass through the intermediate form of creatine and inosic acid, before appearing as uric acid. According to Lehmann, if uric acid be taken into the stomach of a healthy man, it is followed by an augmentation of urea and oxalic acid in the urine; and the same thing may be accomplished by decomposing uric acid by perox. lead. This substance is less influenced by the quality of the food than urea, and is always increased relatively to this principle by those causes that interfere with the oxidation of the tissues.

The ratio that it bears to urea in normal urine, is as one to thirty, and to the other solids, as one to sixty. After taking an indigestible meal, it oftentimes becomes absolutely increased, bearing in the first ratio as 1 to 23, and in the second, as 1 to 41. Consequently, it has been shown that in those diseases termed obstructions of the portal circulation, as well as hæmorrhoids, it is absolutely increased. This is also the case in that group of symptoms designated fever; the acid or its salts, either separating and forming a deposit after voiding the urine, or remaining dissolved in it. Gornod found a constant and

well-marked diminution of uric acid before the paroxysms of both acute and chronic gout. But in these instances there was always an abnormal quantity in the blood. In rheumatism, the reverse is the case, the excess being in the urine, and the deficiency in the blood. "We cannot, however, explain gout by attributing it to this excess, which latter is merely owing to deficient oxidation of the elements of the blood. There is no acute, and but few chronic diseases in which this oxidation is not deficient; and moreover, there is no disease which owes its accompanying symptoms to either a too sudden or too rapid oxidation of the blood."*

But it is important to know that a deposit of this acid or its salts does not always occur because there is an excess. Indeed, such is never the case from excess alone. To be excreted from the blood at all, it must be in solution, and as it is then removed from the laws of vitality, and free to be influenced by chemical reaction alone, the cause of deposition whilst in the urinary passages must be looked for either in the composition of the urine itself, or in the membrane over which it passes. The forms in which this substance is found to exist as a deposit are urate of soda, urate of ammonia, and more rarely urate of lime. Now ammonia, as is well known, is not a constituent of healthy urine, but results from decomposition of the urea, either before or after emission. The existence, therefore, of urate of ammonia implies that decomposition has taken place after secretion. This decomposition is more likely to occur in the bladder than elsewhere, and hence calculi of urate of ammonia should be most commonly found in this viscus. Such in reality is the case, and I am not aware that any having this composition have ever been reported as renal calculi. Urate of soda, on the other hand, is the normal condition in which uric acid exists in solution, and if ammonia alone be produced by decomposition, the urate will be found in this form. Again, if from decomposition a stronger acid than the uric be developed, this acid will unite with the soda, and the result will be a deposit of uric acid alone.

It is exceedingly rare for urine on its emission to contain free uric acid, or urates as a deposit, although the contrary is quite commonly received, the mistake being made by examining the specimen a short time afterwards, when in many cases changes in the relations of its elements have taken place. These changes are produced by the metamorphosis of the pigment into lactic acid, and sometimes also acetic, by the influence of the mucus of the urinary passages. Sherer was the first who drew attention to the subject, and he has proved almost be-

* Lehmann, vol. i., p. 219.

yond doubt that such is the case. He terms the process, acid urinary fermentation. Every healthy man's sedimentary urine, exposed to an ordinary temperature, begins after a longer or shorter time to separate uric acid, and have a more acid reaction. Faintly alkaline urine, such as is passed after vegetable food rich in alkalies, acquires after a time an acid reaction, which may increase so much as to clear up any turbidity occasioned by phosphates, and produce deposit of uric acid. He says this fermentation extends from 4 to 5 days, although under a low temperature it may increase for three weeks, and not subside for six. A very little observation will prove the correctness of these facts, and also of the cause; for, if perfectly fresh urine be boiled, or have alcohol added to it, or still better, filtered, which causes either prevent fermentation or remove the ferment, the changes above described do not take place. Now, if the quantity and quality of the mucus, instead of being normal, be altered in one or both respects, the same results may be produced, before the urine has left the bladder.

We must thus conclude that deposits of uric acid alone, are not only not pathological symptoms, but do not even evidence that the urine contains this principle in excess. And even when passed from the bladder, already deposited, it merely evidences that fermentation has taken place within the viscus. We cannot recognize, therefore, any such disease as a uric acid diathesis, inasmuch as an increased amount is merely due to a simple departure from ordinary physiological laws; and a deposit, to changes taking place in effete organic matter.

The next ingredients after uric acid, that we find enter as constituents into urinary calculi, are the *phosphates*. Of these I shall have less to say, as the causes producing their deposition are more apparent than of the others. The quantity of phosphoric acid and phosphates eliminated by the kidneys has been investigated carefully by several observers—principally, however, by Mosler, Breed, and Winter. The first of these estimates the daily healthy average of phosphoric acid at 49 grains; the second, as high as 61 grains; while Dr. Hammond, in his paper already quoted, averages them only at 43.7 grains. This acid exists in the urine partly as *phosphate of soda*, and partly as *phosphates of lime and magnesia*. Mosler states that the relation of these to one another in a state of health is as three to one, or 36.5 grains of the first, to 12.5 of the other. Both Breed and Mosler ascertained that the quantity of phosphoric acid secreted was greater (both absolutely and relatively) during the waking hours than during sleep, particularly during the evening and afternoon; next in order stood the mid-day and night urine; while that passed in

the morning always contained the least. Mosler states, also, that the phosphoric acid in combination with the earth, is to that in combination with the alkalies, as 1 to 2 in the morning urine; as 1 to 4 in that passed at mid-day; and 1 to 3 at night.

The results obtained by Dr. Hammond are as follows:

Normal amount of phosphoric acid	43.7 grs.
Normal amount of phosphoric acid under increased intellectual efforts	67.2 grs.
Normal amount of phosphoric acid under diminished intellectual efforts	25.4 grs.

Not much additional information in regard to disease, or the amount of phosphates, has been added since the appearance of Dr. Bence Jones' work on Animal Chemistry, published in 1850. The conclusions he there arrives at are, that neither the earthy nor alkaline phosphates are increased in spinal diseases, nor in fevers or acute inflammation of fibrous, muscular or cartilaginous tissues. That in chronic diseases where the nervous tissue is unaffected, in chronic mania, melancholia, and general paralysis of the insane, no marked results are perceptible. That in chronic diseases of the brain, and in both chronic and acute affections of the membranes, there is no increase in the total amount of earthy and alkaline phosphates. That in acute diseases of the brain there is an excessive amount of these salts in the urine, but when the inflammation becomes chronic, no excess is perceptible. Finally, that in some functional diseases of the brain an excessive amount of phosphates is observable, and this ceases as the delirium passes off. Delirium tremens shows a remarkable deficiency in the amount of phosphates excreted, provided no food is taken. When food can be taken, the diminution is not apparent.

The sources from whence phosphoric acid is derived, are manifestly the food, and the normal disintegration of tissues. The proportions in which it unites with soda, lime, and ammonia vary with the amount of these latter present. And as it has been found increased only in one class of disease, namely, inflammation of the brain, and this from the increased waste of phosphorized tissue merely, there can be no ground for assuming any such morbid condition as a phosphatic diathesis. But the causes of their deposition are the subject matter of interest at the present time. However large their amount may be, a very feeble acid is sufficient to retain them in solution, and on the other hand, the whole is often precipitated directly, if the reaction of the urine be alkaline. This latter occurrence is exceedingly common in persons who, from paralysis or other causes, have lost the ability to emp-

ty their bladders, or who are affected with chronic inflammation of this organ. In the latter instance an undue amount of altered mucus is secreted, which, acting as a ferment upon the urea, produces as a result ammonia, by which the acid reaction of the urine is removed, and the phosphates at the same time deposited. It is curious again to remark, that phosphatic calculi are rare, if they ever appear at all, out of the bladder—renal calculi being almost entirely composed of oxalate of lime and uric acid. We must, therefore, look for the causes of phosphatic calculi almost always in the bladder alone.

I come now, lastly, to the consideration of *oxalate of lime*. This salt has never been studied with the minuteness and detail that have characterized the other ingredients of healthy urine; partly because its amount is always small, partly because its detection is not so easily accomplished, and principally, because it is only within a few years that it has been recognized as a healthy, and not a morbid ingredient of the urinary secretion. Not many years have elapsed since pathologists recognized in a few octahedral crystals the existence of a definite morbid condition; and there are few of us who did not see, as we thought, in the results arrived at by the use of nitro-muriatic acid, a confirmation of these pathological views. But that oxalate of lime is an ingredient of healthy urine, and exists in solution, may be readily proved, either as Lehmann suggests, by exposing a healthy urine out of doors to a temperature just below 32°—by this means the water alone freezes slowly, the urine concentrates slowly, and the crystals are found in the deposit in the form of octahedra—or again, evaporate to dryness any specimen of healthy urine, add alcohol to the residue, and filter it. After ten days, I have invariably found octahedral crystals deposited, which could only be oxalate of lime. Moreover, that these crystals, which are exceedingly common in urine under all circumstances, are not derived from the mucus of the bladder, as has been asserted, may be proved by filtering fresh urine. After it is cool, they may often be found deposited, although undetected before. Admitting, therefore, the existence of oxalate of lime in healthy urine as an ordinary constituent, we are to inquire into the causes of its deposition in the form of a sediment or calculus. This, unfortunately, it is not easy to arrive at, for the reasons I have already mentioned. Certain it is, however, that crystals of it are most commonly found after the use of vegetable food, particularly of such as contain ready-formed oxalates. It has been found as a deposit very often after the use of sparkling wines, or beer containing much carbonic acid. But if taken into the stomach by itself, it does

not pass out as such. Lehmann's view, the one most generally received, is, that an increased amount is dependant on disturbance in the respiratory function and imperfect oxidation of certain substances in the blood. Nervous influences which affect this function also tend to produce an increased amount, and its presence in the urine may in part be explained by its formation from uric acid, but it must be recollected that there are also other substances from which it might be formed as well. However, with these facts, there is no need to consider its presence as indicative of a special diathesis, more particularly as the diathesis is assumed to exist from the presence of a single symptom, which is contrary to and opposed to all rational medicine.

Having shown thus far, that uric acid, the phosphates, and oxalate of lime are healthy constituents of urine; that the existence of a deposit of either of them by no means proves that they are in excess, and may even be coincident with a diminution; and, moreover, that there can be no such condition as was formerly assumed, and as even now is often spoken of, namely, an oxalic, a uric, or phosphatic diathesis; it remains to glance for a moment at cystin and cystic oxide, which occasionally, though rarely, are found existing as urinary concretions. Of these, however, we know scarcely anything. A few rare cases have been observed in which a sediment of cystin was found in the urine. Coexistent with this deposit, was a broken-down state of health, and nearly always scrofula. But no one has assumed that these phenomena bore to each other the relative cause and effect, and although various speculations have been enunciated to explain the pathology of these cases, the question is still involved in as much difficulty as at first. Of cystic oxide, nothing more is known than that in some 4 or 5 cases calculi have been extracted, which differed from all other calculi in their appearance and chemical reactions. We therefore may conclude that if there be no special diathesis in the cases where uric acid, the phosphates, and oxalate of lime are found, there are none in the two just mentioned; and, moreover, that the causes which produce a sediment, or calculus, are similar in both instances.

Having now dismissed the subject of diathesis, the most important argument to combat, let us next examine the various statements that have been noted by careful and intelligent observers, and see what bearing they have upon the subject under consideration.

Dr. Gross has added, in the form of an appendix to his work on "Diseases of the Urinary Organs," some important facts in reference to the frequency of calculus in various parts of the world, more par-

ticularly the United States. From the information there embodied, I am enabled to arrive at the following conclusions:

Those portions of the United States where stone in the bladder is most frequently found, are Kentucky, Tennessee, Virginia, Ohio, and North Alabama.

In Indiana, New York, Pennsylvania, Maryland, the New England States, Canada, Texas, and California, it is very unfrequent, in the four latter places being scarcely known.

Moreover, that in all these places indiscriminately, without relation to the frequency or infrequency of calculus, urinary deposits of all kinds are exceedingly common.

I learn from other sources that in England, Holland, and the North-eastern part of France, this disease very frequently occurs, probably more so than in any portion of the United States. While in Switzerland, Germany, and the lower part of France, it is very rarely found. In regard to the rest of Europe, India, &c., very contradictory statements only can be obtained.

Moreover, that seamen are scarcely if at all liable to it; and this is true not only as regards our navy, but, on the authority of Dr. Hutchison, of England, it is a rare disease among the commercial seamen of Great Britain.

It is also stated by Dr. Gross, that the negroes of Kentucky and Tennessee are very seldom operated on for stone in the bladder.

So much for localities. In reference to habits of living, we find that it is almost entirely an affliction of the lower classes, the better being comparatively exempt. That beer or cider drinking, which have been looked upon as causes, if they produce any effect, rather act as preventives, as the German population of this country who drink beer, and the New Englanders who drink cider, rarely suffer from it. That gin drinking, which has been assumed to have some effects, is of little importance, as this habit is confined to Holland alone; and moreover, the Hollanders, who have carried this habit with them to Batavia, are there entirely exempt from the disease.

Great stress has been paid to geological peculiarities, particularly limestone districts; and it has been supposed that the use of water impregnated with calcareous salts, offered a solution of the question. But here again we find that Holland, England, and the Northeastern part of France are not lime districts; that New York and many parts of New England abound in this formation; and that, according to Dr. Gross, the cases afforded by Kentucky and Tennessee are

found about equally divided in the limestone districts and those free from this mineral.

A comparison of the diet used in the various countries under consideration affords no better solution; nor can anything be said in reference to particular latitudes, but that the disease is much more common in temperate regions than in northern countries or the tropics.

There is one other condition, however, which has not yet been alluded to. This is the hygrometric condition of the atmosphere. It has already been suggested by Dr. Copland Hutchison, that the comparative exemption of sailors might be explained, among other causes, "by their exercising the functions of respiration with unusual vigor," and "by the copious perspiration produced in the sailors on board ships of war, owing to the narrow space into which their hammocks are crowded during the night." I cannot but think that this suggestion of Dr. Hutchison is entitled to more weight than as yet has been accorded to it. Nor would I propose a chemical explanation, such as has been offered. It has been said that, by the free action of the skin, excrementitious matters are gotten rid of, which, if retained, could only be eliminated by the kidneys in the form of uric acid, urate of ammonia, &c., thereby producing an excess of those ingredients in the urine, and consequently a tendency to deposition in the form of calculus. I have already shown that mere excess of any particular ingredient in the urine has no effect in producing a deposit; and in addition, that urinary sediments are quite as frequent in non-calculous districts as those where it is most commonly found. Some other cause, then, must be looked for to explain this affection.

Of all places in the world, England, Holland, and the Northeastern part of France are notoriously those in which the humidity of the air exists to the greatest degree. There is a striking contrast between the moist damp atmosphere of those places, and the clear sky and dry air of Switzerland, Germany, and the South of France. This is also proved by the dew point, which, by direct observation, is found to be higher in the first mentioned places than the others. We have not the same exact data to rely upon in reference to our own country, but the following facts, which I gather from Dr. Drake's book on the "Climate of the Valley of the Mississippi," are of exceeding importance, as far as they go, in bearing out this same view. Thus, the average annual amount of rain in inches in the places mentioned below, are as follows:

Mobile	66.9	Fort Leavenworth	32.6
Natches	60.1	New York	33.3
Huntsville	64.8	Lake District	29.3
Nashville	55.0		
Average	61.1	Average	31.9

The first of these being, as I have shown from Dr. Gross, the stone districts, while the latter are comparatively exempt. Again, Dr. Drake makes the following statements in regard to the moisture of the air in the various districts, as ascertained by the dew point.

West of the Gulf of Mexico and Mississippi, very dry.

Lake Region, very dry.

Arctic Region, scarcely any moisture.

East of Mississippi, particularly Tennessee and Kentucky, very moist.

A friend has also directed my attention to a statement in Dr. Livingston's book, in which he says that in the interior of Africa the climate is remarkably dry, and the air free from moisture, while at the same time calculous diseases are entirely unknown.

I think we may conclude, therefore, that a moist climate does have the effect of producing urinary calculus; and the next question is, how does it effect it?

In the first place, an undue amount of moisture in the air interferes with the action of the skin, and predisposes individuals living in such a climate to congestion and inflammation of the mucous membranes. Of the important rôle it plays in the production of catarrh, bronchitis and pneumonia, I need not speak; that it influences also the lining membrane of the urinary apparatus in the same manner, although not to the same degree, we can readily understand; and we have then as a result, the mucus and epithelium secreted by this congested membrane altered either in quantity or quality, or in both.

Secondly, a much larger proportion of every calculus consists of animal matter, than is generally supposed; and Dr. Haskins, of Kentucky, states that if a small quantity of calculus matter imperfectly pulverized, and partially dissolved, be placed under the microscope, the particles thus examined, when completely divested of salts, will be found to bear so great a resemblance to epithelium scales, as to be easily mistaken for them. Moreover, in examining the centre of most calculi, it is found (particularly of late years, for previously the diathesis was considered all important,) either that the nucleus consists of a clot of blood, mucus or epithelium, or that there is a small central space produced by the contraction of these substances, after the

calculus has formed around them. And to quote authorities, Prof. Sharling, of Copenhagen, says that, "if we attentively examine any of the fissured or perforated calculi so often met with, or in which a central mass of crystals replaces the usual nucleus, we shall have evidence of the manner in which a clot of blood, a flake of mucus, or albumen detains the salifiable ingredients, the hydrates as it were, and forms the elements of a nucleus, which consolidates, and in its turn constitutes the centre for future deposition."

Thirdly, all observers agree that stricture of the urethra, disease of the prostate, and organic disease of the kidneys, ureters and bladder, have a decided effect in producing calculi. In all these cases the mucous membrane itself is more or less altered. I need but allude for a moment to the common occurrence of foreign substances, introduced from without, constituting a nucleus, and how inevitable it is that a calculus should be formed when such substances are present. Now, I have already alluded to the fermentation that ensues in urine, by which changes in its acid and alkaline reaction are induced, and which will explain all or nearly all the deposits that are found in the urinary secretion. We have also seen that the frequency of these deposits in different places bears no relation to the frequency of calculi. Moreover, that in a majority, if not in all calculi, the nucleus, as well as a large portion of the calculus itself, consists of animal matter; that these concretions occur most frequently in moist climates, and that such climacteric conditions induce a morbid secretion from the urinary mucous membrane.

Do I go too far, then, in attributing to this morbid secretion the necessary element for the production of calculus? What this consists of, whether blood, mucus, epithelium, or albumen, it is hard to say; whether congestion, acute or chronic inflammation, we do not know. But admitting the fact, that it is to the mucous membrane we are to look, and not to the composition of the urine, as the cause of calculi, will we not gain more by addressing our treatment to the urinary passages, than to some fancied excess or deficiency of its component elements? I am aware that the question I have been discussing at so much length, is more scientific than practical; but I am led to think that it is better to take a correct view of a pathological condition, even though the results be only of importance indirectly, than to grope on in error, taking for granted what other men have written, instead of inquiring for ourselves.

The Physiological Researches of Dr. Brown-Séquard. By E. R. PEASLEE, A. M., M. D.

The readers of the MONTHLY have already had their attention called to some of the researches of Dr. Brown-Séquard, and the conclusions he has based upon them; but, so far as we are aware, no complete abstract of them has yet appeared in the English language. Such an abstract is therefore here attempted.

Of course, entire justice cannot be done to this most distinguished of experimental physiologists, by a mere resumè; but the great interest and practical value of his labors demand this, at least, for the benefit of our readers, and the profession generally. We shall therefore confine ourselves mainly to the results of his researches, and their practical applications, to the omission of most of the experimental details. We derive the materials for the following abstract mainly from a notice of his works, published by Dr. Brown-Séquard, two years since; and shall, at another time, call attention to his more recent investigations.

I.—*Dr. Brown-Séquard's Experiments on the Nervous System.*

1. THE SPINAL MARROW, (INCLUDING THE MEDULLA OBLONGATA,) AND ITS NERVES.

I.—A *division of one lateral half of the Spinal Marrow*, opposite the last dorsal or first lumbar vertebræ, is found to produce the following singular changes of nutrition. (1.) The cornea, on the side operated upon, quite frequently becomes ulcerated; and in other cases becomes opake to a greater or less extent. These alterations, however, usually disappear rapidly and spontaneously. (2.) The suprarenal glands become congested after the section; and at the end of several months, they are found hypertrophied, sometimes to two or three times their natural size. (3.) The posterior extremity, on the side of the section, manifests phenomena like those in the face, which follow the section of the great sympathetic nerve in the neck, viz., dilatation of the vessels, elevation of temperature, increased muscular irritability, and a lengthened duration of it, and of the excitability of the motor nerves after death; a later appearance and a longer continuance of the cadaveric rigidity, and a later development and slower progress of the putrefactive process. These last phenomena show that the nerves of the blood vessels of the extremity are continuous into the spinal cord, and have been divided in the section.

II.—The effects on the intensity of the *reflex* (diastaltic) function of

the cord by a *transverse section* entirely through it, are very remarkable. The following occurred in a frog, whose spinal cord was divided on a level with the 3d pair of nerves; the voluntary strength of the posterior extremities being sufficient to raise 900 grains $\frac{1}{12}$ of an inch.

(1.) Immediately after the section the motor force is sometimes null; but generally is reduced to $\frac{1}{4}$ or $\frac{1}{3}$ its normal amount. It never remains unchanged. (2.) Five minutes after the operation the motor force is from $\frac{1}{3}$ to $\frac{3}{4}$ its normal amount; in 15 minutes, from $\frac{1}{2}$ to even its original amount; and in 20 to 25 minutes it is generally the same as before the operation. (3.) In an hour the force is sometimes doubled, but generally not increased to this extent; in 2 or 3 hours it is doubled, and sometimes tripled; and in 24 hours arrives at its maximum, though it augments very slowly after the first 3 hours. (4.) It remains almost stationary, after attaining the maximum, for 6, 10, 15, or 20 days, and then gradually decreases, so as to be reduced, if the frog lives 6 to 8 months, to $\frac{1}{2}$ or $\frac{1}{3}$ its original amount.

But by powerfully exciting the reflex action many times, in an animal whose spinal cord has been transversely divided behind the brachial enlargement, the reflex functions may be made to disappear almost entirely; though the power is again nearly restored by perfect repose, in 15 to 20 minutes.

A transverse section *of all the white substance* of the spinal cord in the cervical region, does not destroy the sensory communication between the encephalon and the various parts of the body; the gray central substance still conducting sensory impressions. This, Dr. B-S. believes, proves that all the sensory fibres in the nerve-trunks are not continuous into the encephalon. But he maintains that each sensitive fibre, which really does ascend from the cord to the encephalon, represents several fibres from the sensitive surfaces or parts; being continuous with them, through the medium of cells or otherwise.

III.—Dr. B-Séquard concludes, from his experiments, that the fibres of the posterior or sensitive roots of the spinal nerves divide, on entering the cord, into two series, *ascending and descending*. The *ascending* pass towards the encephalon, (1) after penetrating the posterior columns, or (2) in the interior of the posterior gray cornua, or (3) after entering the lateral columns; and soon plunge into the central portion of the gray matter of the cord. The *descending* fibres pass through the same parts in the opposite direction, and also terminate in the central gray matter. The sensory impressions brought thus to the gray matter are transmitted either *by* or *through* the latter to the encephalon. There is a third class of fibres in the posterior

roots of the spinal nerves—the *transverse*. These are probably *not sensitive* fibres.

Thus Dr. B-S. holds that sensory impressions are at first transmitted, some upwards and some downwards; but all reach the central gray matter, which in its turn transmits them to the encephalon.

[It is very generally taken for granted that the gray matter of the cord consists of mere cells and blood vessels, without an admixture of nerve fibres. It, however, really has one half or more of its bulk made up of nerve fibres. These also are mere continuations of the fibres in the sensory roots, penetrating to the central gray matter, as Dr. B-S. has demonstrated. These *fibres* therefore in the gray matter, and not the latter merely as gray (or cellular) matter, conduct the sensory impressions to the encephalon; they being also continuous to the latter, as is explained in my work on Histology, p. 457–9. E. R. P.]

Dr. Brown-Séquard has also shown that sensory impressions are transmitted principally *across* the spinal marrow, i.e., from the sensory nerves on one side, through the opposite half of the cord to the encephalon. After transversely dividing one half of the cord, he found that the posterior extremity of that side is even more sensitive than in the normal state, while the limb on the opposite side is less sensitive than before. Thus we explain the fact, that anæsthesia on one side of the body often results from injuries upon the other, i.e., of the opposite half of the nervous centre; and whether the lesion be in the cord or the encephalon, the result is the same. Dr. Brown-Séquard concludes that most, if not all sensory impressions are conducted across to the opposite side of the cord, and that the crossing is generally effected immediately on entering the cord; while in a few instances it occurs above the point of entrance of the nerve fibres, and in a few also below this point.

[The crossed direction of the nerve fibres entering the cord, thus inferred by Dr. B.-Séquard, has also been rendered probable by the observations of Kölliker, as shown in the work on Human Histology, (p. 456,) above referred to.]

While, however, the loss of sensation consequent on a lesion of one half of the nervous centre *always* occurs on the *opposite* side of the body, wherever the lesion may be—a loss of voluntary *motion* produced by such a lesion may be on the opposite or the same side, or on both sides, according to the precise seat of the lesion. Dr. B-Séquard has shown that if the lesion is seated anywhere above the lower part of the medulla oblongata, the loss of motion as well as

sensation occurs on the opposite side; while if it implicates the medulla oblongata at the decussation of the anterior pyramids, the loss of motion will effect both sides, and that of sensation the opposite side only; and if below the medulla, (i.e., in the spinal marrow,) the loss of motion occurs on the same side, and that of sensation on the opposite. [These facts are at once seen to be of the utmost importance in diagnosis.]

In regard to the *different degrees of sensibility* in different portions of the sensory nerve fibres, Dr. B-S. finds the highest sensibility just where these fibres enter the cord in the posterior roots; the next lower degree in the papillæ of the skin; next, in the posterior roots themselves; and last, in a trunk uniting all the fibres, from a certain surface of the skin. On the other hand, the same fibres manifest no sensibility at all after entering the gray central substance of the cord. He finds that the trifacial nerve loses its sensibility after passing a very short distance into the encephalon, and that sensitive fibres (in the rabbit) lose their sensibility while passing through a spinal ganglion, and recover it on emerging again therefrom. This fact, he thinks, demonstrates that the "power of transmitting sensitive impression may exist in a part which has not the power to receive them." [We suggest that the term *impressibility* may be more accurate than *sensibility* in the preceding statements; and that the different structural relations of the nerve fibres in different parts of their course may explain the facts just related.]

Dr. B.-Séquard has shown, that the direct exposure of the spinal cord to the air is, in all the mammiferæ usually experimented on, entirely without danger. Facts also warrant the same conclusion in respect to man. He therefore proposes to trephine over the cord for the removal of spicula of bone or depressed portions, in all cases (as of fracture of the vertebræ) requiring this operation. He also found that when he removed the posterior arches of several of the vertebræ, (in several species of animals,) they were reproduced, the new portions being generally wider than those removed, and often adhering together. They generally left the spinal canal of the normal diameter.

IV.—In regard to the *regeneration* of the nervous tissues, M. Flourens has demonstrated, that wounds of both the encephalon and the spinal cord may cicatrize. Dr. B.-S. has demonstrated that the voluntary motions, and sensations, may return after a complete transverse section of the spinal cord. He has also seen a case of complete return of voluntary movement, and of sensibility, after the division of

the sciatic nerve. A microscopic examination with M. Lebert, demonstrated that the nerve fibres had re-united.

V.—In regard to the functions of the *medulla oblongata*, Dr. Brown-Séquard has arrived at the following conclusions:

I.—*Its influence on the nutritive functions.* M. Flourens found that the removal of even a small part of the medulla oblongata in the higher vertebrata, at their ordinary temperature, immediately produces death. Dr. Brown-Séquard's experiments have led him to the following results: (1.) In autumn and winter, frogs will live several weeks after the removal of the medulla only, or of this and the rest of the encephalon—of the portions of the latter in front of the medulla—or of the whole encephalon, together with the portion of the spinal marrow which is in front of the roots of the 2d pair of nerves. (2.) Frogs may live one or two weeks during the cold season with one-half, one-third, or even one-quarter of the spinal marrow; all the rest of the cerebro-spinal centre being destroyed. They also manifest most of their organic functions. (3.) The spinal cord seems more useful, in relation to the organic functions, than the medulla oblongata and the rest of the encephalon. A portion of the former, (that which gives rise to the 2d and 3d pair of nerves,) contains less gray matter than the medulla, but nevertheless maintains life as long, or even longer, than it does. (4.) All parts of the cerebro-spinal centre, except the lobes of the cerebrum, seem subservient to the organic functions. And whatever part is removed, life ceases within from a few days to five weeks; while whatever part is left alone, life continues at least three days, and generally longer.

The maximum duration of life, after the entire removal of the medulla oblongata, varies with the species, as the following table will show:

FISHES.	Eels	lived....6 hours.
	Carp, Tench, &c.	3 "
	Perch, &c.	25 to 40 "
AMPHIBIA.	Salamanders, Frogs,	more than 4 months.
	Toads	4 to 5 weeks.
REPTILES.	Tortoise	9 to 10 days.
	Lizards	4 to 6 "
BIRDS.	Sparrow-hawk, Magpie, Sparrow,	just hatched....17 to 21 minutes.
	Linnet, Pigeon, Hen, Duck, &c.	1 to 2 "
MAMMALS.	Hedgehog, (while hibernating)	23 hours.
	Puppies and Kittens, (new born)	46 to 41 minutes.
	Hedgehog, (in summer)	4 "
	Dogs, Cats, Rabbits, Guinea Pigs.	3 to ¾ "

From the preceding it appears that after the removal of the medulla oblongata, the duration of life is estimated in frogs and salamanders by *months;* in some reptiles by *weeks;* in other reptiles and in fishes, by *days;* in certain animals, while hibernating, by *hours;* and in birds and mammals, by *minutes* merely.

In frogs, deprived of the medullao blongata, Dr. Brown-Séquard finds, (1,) that the circulation of the blood continues apparently as before, except that the contractions of the heart become more frequent during the first half-hour after the experiment. This demonstrates that the cardiac contractions do not depend on the medulla oblongata. (2.) That the contractions of the four lymphatic hearts also continue normal. (3.) That digestion is also not interfered with; so that it does not (in frogs) depend upon the pneumo-gastric nerve. (4.) That the secretion of urine, and the production of epithelium on the skin and mucous membranes, still continue normal. (5.) That aëration by the skin, as well as absorption of water or poisonous substances, continues as before. (6.) That the reflex (diastaltic) function of the spinal marrow becomes so much augmented, that frogs, operated upon, move far greater weights than they can overcome by voluntary efforts. Of course, then, the nerves and the muscles retain their vital properties. (7.) That the galvanic current in the muscles becomes stronger. (8.) That the vital properties of the cord, the nerves, and the muscles are so augmented, instead of being diminished, that on etherizing an animal experimented upon, or on removing the heart, these properties remain longer than in the normal condition. (9.) In all animals, life continues longer the lower the temperature. (10.) It is not the fact, however, that the animal experimented on lives longer at a low temperature, merely because it expends its vital force more slowly; the aggregate of the vital phenomena being the same as at a higher temperature. For the frog lives in winter, when the heart beats thirty-five times per minute, for four months, a period requiring more than six million of beats; while in summer, with forty-five contractions per minute, it lives but six hours, a period including but sixteen thousand two hundred beats, or $\frac{1}{375}$ of the former number. There is, therefore, a far greater exertion of vital force in the former than in the latter case.

On the other hand, respiration by the lungs at once fails on the removal of the medulla oblongata; it having been shown by M. Flourens that a very limited portion of it is the centre of this function. And the reason why life continues so much longer after its removal, at a low temperature, is, that the resistance to asphyxia is thus proportionably increased.

II.—The *auditory nerve*, rising from the medulla oblongata, is regarded by Dr. B-Séquard (with M. Flourens) as in part a *nervous centre*, while the rest is merely the nerve of audition. Puncture of this nerve causes the animal (mammals) to roll upon the longitudinal axis of the body, as long as they live after the injury. In frogs (1) the nerve is exceedingly sensitive; (2) puncture of it produces a remarkable state of hyperæsthesia; (3) the fore-leg of the opposite side is thrown far from the body in the act of extension, (semi-paralyzed and semi-convulsed;) (4) the animal turns or rolls around its longitudinal axis, and generally to the side affected.

Turning and rolling movements are also produced by puncture of the medulla oblongata; and that of one of the halves of the spinal cord, near the medulla, produces turning. The evulsion of the two or three first roots of the spinal nerves also produces turning; and that of the facial nerve both turning and rolling. Finally, (1) turning to the *left* may be produced by a lesion in the immediate vicinity of another which causes turning to the *right*. (2.) A lesion at first producing rolling, may, after a while, cause turning only. (3.) The same lesion may cause rolling on one side and turning on the other. (4.) In animals which turn or roll, certain muscles (and especially of the neck and neighboring parts) are always in a state of clonic convulsion; and on this state these two motions mainly depend. Thus certain nerve fibres, not subservient to voluntary motion, probably rise in different points of the encephalon, and are distributed to the muscles, both of the same and of the opposite side.

Carate, a non-classified Disease of the Skin. By G. VAN ARCKEN, M.D. San José de Cucuta, New Grenada.

To the Editors AMERICAN MEDICAL MONTHLY.—Enclosed you will find a paper from one of my correspondents in Central America, on certain forms of Dermatopathia.

Hoping it may contribute to our knowledge of diseases incident to tropical climates, I trust you will give it an insertion in your valuable journal.

Respectfully yours,

NEW YORK, *March 10th*, 1858. E. H. DAVIS.

The subject of diseases of the skin has been well nigh exhausted by Willan, Bateman, Rayer, E. Wilson, Cazenave, Schedel, Biett, Plumbe and other dermatological writers. But the disease known in South America by the name of "*Carate*," has either been overlooked, or else has not yet come to their notice. I have not been able to

find the slightest mention made of this disease, in all the works on dermatology which I have consulted.

This has caused me to publish the results of my investigations, hoping thereby to explain as far as possible the nature of this loathsome disease, which is considered incurable by the natives; but which, according to my experience, admits in most cases of a complete cure.

Classification.

The various diseases of the skin, such as they have been described by medical writers, are extremely numerous. Almost every author arranges a system of his own, in which he frequently carries his nicety of discrimination too far. A multitude of names and distinctions is hardly ever of any practical advantage.

However carefully an author classifies the diseases of the skin, arranging them according to their symptoms, the course they pursue, and their various complications, there always remain some affections, for which there is no room left, and which cannot be associated with any class.

Never have I been compelled to appreciate this fact so highly, as when I tried to bring the subject of this treatise under the head of some class of diseases of the skin, or rather to find a name for it. According to its appearance, it might be brought under "*Discolorations of the Skin, or Maculæ;*" but Maculæ have their seat and cause essentially in the skin, and Carate is a constitutional disease.

Some few cases look at first sight like *Roseola*, but Carate is decidedly a chronic disease; commences very insidiously, and is never accompanied by any trace of fever whatever.

The blue variety might be mistaken for "*Pityriasis versicolor*," but Carate shows none whatever of the symptoms of the scaly diseases; it does not desquamate, and becomes stationary at the end of a few years.

These and other reasons have determined me to put Carate down as a non-classified disease of the skin, and also not to change its name, which is composed of the Spanish "Cara," which means *face*, and the Indian "Ate," which means *look*, giving therefore the complaint in plain English, the somewhat original name of "*Look at his Face.*"

Description and Varieties.

By "*Carate*" is understood a change of the skin from its natural appearance to one more or less abnormal in color.

There are three varieties of this disease, namely: the blue, white and rose-colored. The first is never complicated with either of the latter, but the last two frequently coalesce into one.

Blue Carate is the mildest form. It generally attacks persons between 15 and 25 years of age, without any distinction to race or color; commencing with the face, and especially those parts which are but slightly covered with integuments. The discolorations, generally of a leaden hue, may be compared to those which appear after a long-continued internal use of the nitrate of silver.

When the disease first breaks out, the spots are round or oval, but they soon enlarge and become very irregular. From the face the discolorations extend down to all the parts of the neck and chest. The course of the ribs is frequently so distinctly marked, that the patient looks like a Zebra, or "*âne rayé.*"

The arms remain generally free, but the hands are the favorite seat of the disease, and upon these it exhibits all shades and hues. The lower extremities also remain tolerably free, excepting the lower third of the *tibia*, which again is a favorite seat of this disease.

This variety sometimes makes its appearance on the *glans penis*, but the female organs remain free from it. Blue Carate appears to attack the male more frequently than the female; at any rate, where it attacks females, it is in a very slight form, and gives way in a few days, under the influence of appropriate medicines.

The second variety, or white Carate, is seldom found in the male, and may therefore be almost considered an especial disease of the female.

It is frequently accompanied by sterility, inflammations of the uterus, of a chronic character, and the various diseases to which the ovaries are subject.

This variety makes its first appearance on the neck and chest, and extends from these over the face and abdomen. It rarely appears on the extremities.

The color is of a dead chalky white, and makes a very disagreeable impression on the spectator. It attacks people mostly from 30 to 40 years of age, and becomes sooner stationary than the blue variety.

Rose-colored Carate is the worst kind, and frequently follows an attack of the white variety, in which case there appears, on the dead chalky spots of the latter, some very minute red points, which gradually enlarge until the whole assumes a pale red color.

Those affected with this disease are mostly Sambos, Mulattoes, half-breed Indians and others of a dark complexion, for which reason the difference of color is the more striking.

This variety always commences first on the hands, extends from these to the face and neck, and then down again over the abdomen.

Attacking both sexes with equal frequency, the favorite seat of this disease is, in the male sufferers, the palmar surface of the hands, and in the female, the mons veneris, labia majora, and inner part of the thigh, where it terminates abruptly, extending only in very few instances to the lower extremities.

Locality—Popular Belief as to the Causes of the Disease.

Although some sporadic cases of Carate or some kindred disease may be found in almost all tropical countries, still its true home is the shores of the rivers Magdalena, Zulia and Catatumbo, especially the latter two, which empty into the lake of Maracaibo.

These rivers, winding their way through immense swamps and low-lands covered with virgin forests, abound in insects of all descriptions and in *miasma*, and effluvia of every possible kind.

Some places on the shores of these rivers are so infected, that it suffices for a stranger to stop a few hours there, to receive the germ of a most malignant intermittent fever, which is very difficult indeed to cure.

The people living in the neighborhood of these rivers, and the boat-men who spend their lives in pulling up the flat-boats and bungoes, are those who suffer almost without a single exception from carate.

Their constant exposure to the inclemency of the weather and the bites of legions of mosquitoes, has introduced among them the popular belief, that carate is produced by the sting of an insect, which they call "*carate fly;*" but it is almost useless to say, that nobody has ever seen this supposed insect.

Some few attribute the disease to a sudden change of the weather, which may indeed be a predisposing cause, if we consider the extreme rapidity with which the weather changes in these localities from a most oppressive heat to the most tremendous cold rain storms.

Others, again, attribute it to eating a certain spotted fish, or living on wild hog's meat; but all this is very far from telling us the true cause of this abominable disease.

Rational Causes and Complications.

The lower class of the people, who live on the northern part of South America, spend their lives in the most abject misery, perhaps more so than in any other Spanish American country.

Without any dwelling to shelter them against the inclemency of the weather, going barefooted and frequently entirely naked, eating nothing but plantains and blackberries, too lazy to keep their bodies clean—they are most pitiful specimens of a degraded and brutified humanity.

They come into the world covered with venereal eruptions; the infected milk of their mothers produces ulcers in their throat and venereal warts on the tongue, and from 12 years upwards, they contract primary syphilis and gonorrhœa.

Among these classes of people there exists a popular opinion about "*venereal humor*," or "*humor galico*," as it is called in Spanish, which may be contracted by over-exercise, sleeping exposed to the rays of the sun or moon, fording a river while perspiring, etc. Any one of these causes, according to their opinion, produces all at once a large amount of the so-called humor in the blood; and when they afterwards contract syphilis or gonorrhœa, they consider this an effort of nature to expel the morbid humor from the system, which must therefore be encouraged.

After this, nobody will be astonished to find in this country hundreds of cases of syphilis and gonorrhœa of ten years' standing or more, which are kept up by their unfortunate owners by all possible means. However much they may be told the contrary, they firmly believe, that if the disease were to be stopped, they would die a sudden death soon afterwards.

The natural consequence of this is, that among a thousand people hardly one can be found whose constitution is not tainted with syphilis.

If, then, we take all this together; the frequent exposure to the very changeable weather, the dirtiness of body, insufficient and unwholesome food, syphilis in all its degrees and varieties, this miasma abounding throughout the country, etc., etc., we have a picture of the people who suffer from carate, and of the rational causes of this loathsome disease, whose dormant germ is stimulated into action by the irritation produced by the sting of legions of mosquitoes.

I have never been able to ascertain whether carate shows any disposition to combine with other diseases; all I can say about this is, that out of four persons affected with this disease who died under my treatment, two died of Bright's disease, and that these two cases are the only ones of Bright's disease which have come under my treatment in this country.

Prognosis.

Carate, although considered incurable by the native practitioners, is by no means so. The blue variety can generally be entirely cured by the application of appropriate remedies in about eight weeks. The other two require, on account of their obstinacy, a much longer treatment. But the complete cases, whether they be congenital or contracted afterwards, are better left alone. For, being subject only to a

partial cure, in which case the natural sambo colors would again appear in spots, this partial cure would produce a state exactly opposite to carate, and perhaps more disagreeable to the eye than the disease itself.

Carate never proves fatal of itself, and cannot be communicated to others. But its predisposing causes may be communicated by parents to their children, who may then contract the disease by exposing themselves to those causes which are calculated to call it into action.

Treatment.

The most effective remedies against this disease are the mercurials, especially when administered in alterative doses. Small doses of the bi-chloride, given in a bitter decoction or tincture, produce admirable results; but its effect must be closely watched.

In the more obstinate cases, the iodides must be resorted to, especially the biniodide, which frequently acts like a charm.

In milder cases, the iodide of potassium in the compound syrup of sarsaparilla, proves very effective. The same may be said of the iodide of iron, especially when combined with rhubarb and extract of dandelion.

Still, in some cases, these remedies are not sufficient, and then the preparations of arsenic must be employed.

External remedies are also productive of good results. In mild cases, the alkalies are sufficient, while in the others the sulphurous baths, or those with bi-chloride, must be resorted to. The diet must in all cases be sufficiently nutritious, and a change of residence to a more healthy and airy country is sometimes necessary.

To give a more correct idea of my plan of treatment, I here annex the descriptions of some cases, taken from my journal.

Case No. I.—Maria B———, 19 years old, blue carate, suffered since her 14th year; menstruation very deficient; occasionally leucorrhœa presents itself, but the neck of the womb is not ulcerated.

May 15*th.*—Ordered three successive purges of blue pill and coloeynth.

May 20*th.*—Alkaline baths and iodide of iron, with aloes.

June 7*th.*—Baths with sulphuret of potash; internal medicine continued; exercise on horse-back. The disease commences to give way.

June 18*th.*—Vaginal injections with acetate of zinc. Bathing and internal medicine continued.

June 29*th.*—Small doses of bi-chloride of mercury, in compound tincture of cinchona.

July 16*th.*—The patient is improving; leucorrhœa has not returned, medicine continued.

July 28*th.*—No trace of disease; the general health has improved very much; patient discharged.

Case No. II.—John S———, 31 years old, suffered already 8 years from blue carate. Has had a severe attack of gonorrhœa; the left testicle is swollen and very tender to the touch.

April 6*th.*—Tepid baths; mercurial ointment to the testicle; one scruple of iodide of potassium daily.

April 15*th.*—Continued.

April 24*th.*—Baths with sulphuret of potash; the testicle is well.

May 10*th.*—Iodide of potash augmented; nutritious diet.

May 21*st.*—Small doses of bi-chloride in tincture of gentian.

June 4*th.*—Continued.

June 15*th.*—Iodide of mercury $\frac{1}{8}$ grain daily, in pills.

June 29*th.*—Treatment continued; the patient is almost well.

July 12*th.*—Patient discharged.

Case No. III.—William M———, 28 years old, white carate of 3 years' standing; no complication whatever.

April 21*st.*—Sulphurous baths; $\frac{1}{4}$ of a grain of bi-chloride of mercury daily.

May 2*nd.*—Continued.

May 13*th.*—Bathing continued; bi-chloride increased.

May 25*th.*—Treatment continued.

June 8*th.*—Medicine increased; the disease begins to give way.

June 12*th.*—Salivation commences; medicines suspended; gargles with tannin.

June 18*th.*—Medicine resumed.

June 28*th.*—Treatment and sulphurous baths continued; patient improving.

July 10*th.*—Patient discharged.

Case No. IV.—Casimira A———, 16 years old, suffered about three years from white carate; the disease commences going over into rose-colored carate; scrofulous constitution.

August 2*nd.*—Sulphurous baths; after each a friction with aromatic spirits; cod liver oil and iodide of iron daily.

August 15*th.*—Cod liver oil increased; the other medicines continued.

August 24*th.*—Half a drachm of mercurial ointment to be rubbed every evening on the soles of the feet.

September 6*th.*—Continued.

September 14*th.*—Twice daily one-sixteenth of a grain of bin-iodide of mercury; patient improving.

September 27*th.*—Bin-iodide continued; three times every week a tepid bath, with one drachm of bi-chloride of mercury.

October 8*th.*—Treatment continued; patient rapidly improving.

October 20*th.*—Cod liver oil increased.

November 6*th*—Discharged.

Case No. V.—Louisa G———, 34 years old, suffered since her youth from rose-colored carate; evinces frequent lancinating pains in the left breast; mother died of cancer of the womb.

January 2*nd.*—One scruple of potash daily; sulphurous baths.

January 15*th.*—Iodide increased.

January 28*th.*—No effect whatever; baths, with one drachm bi-chloride.

February 5*th.*—Salivation; medicines suspended; astringent gargles.

February 12*th.*—Distressing cardiac symptoms; demulcent drinks, with small doses of opium.

February 18*th.*—Three times daily ten drops of Fowler's solution; baths, with four ounces impure carbonate of soda.

February 27*th.*—Fowler's solution increased; patient improving.

March 8*th.*—Takes now three times daily twenty drops of Fowler's solution.

March 25*th.*—Every night and morning five drops of Donovan's solution.

April 10*th.*—Medicine increased. The patient is now improving rapidly.

April 22*nd.*—Donovan's solution increased to fifteen drops twice a day.

May 8*th.*—Medicine gradually withdrawn; patient almost cured.

May 20*th.*—Discharged. There is no trace whatever left of the disease.

Case No. VI.—Lucy S———, 28 years old, for a number of years affected with red-colored carate, which is so extensive as to make her look almost like an albino.

May 12*th.*—Baths, with bi-chloride; afterwards frictions with aromatic spirits; twice daily one-sixteenth of a grain of bin-iodide of mercury in pills.

May 25*th.*—Continued.

June 6*th.*—Bin-iodide increased.

June 14*th.*—Salivation; gargles with Labarraque's solution.

June 17*th.*—Better; gargles continued.

June 24*th.*—Bin-iodide taken up again in small doses; alkaline baths.

July 5*th.*—Salivation commences again; mercury laid aside.

July 12*th.*—Iodide of iron six grains daily, combined with extracts of dandelion and rhubarb.

July 14*th.*—Discharged during the night from the bowels an immense quantity of black fetid matter, which looked very much like tar.

July 20*th.*—Rapidly improving; baths continued.

July 28*th* —Iodide of iron increased.

August 15*th.*—Improving: exercise on horse-back.

September 5*th.*—Discharged.

The Jewish Circumcision before a Medical Tribunal. By Dr. JOSEPH HIRSCHFELD. Translated from the *Oesterreichische Zeitschrift für Practische Heilkunde*, *Nov.* 27, 1857, for the MONTHLY. By J. V. L.

A question in Jewish theology has lately been discussed, the decision of which properly belongs to medical surgery. One Dr. Levit, who, as an Israelite, refused to circumcise his child, excuses his refusal among other things, on the ground that the operation is not wholly without danger—that he regards it as a severe injury—as dangerous to life, and even fatal.

It will, therefore, be interesting for the public, and especially for the physician, to bring this opinion of Dr. L. to the proof, and simply to inquire whether the operation of circumcision is shown to be dangerous to life, according to the prognosis given of it by any authority. Upon this point it will be permitted me merely to show that Balassa, Benedict, Dumereicher, Langenbeck, Middledorpf, Pitka, Remer, Schuh, and the other Coryphei of operative surgery, whose clinics I have visited, and in some of which I had the opportunity to stand as pupil, in no manner consider or point out circumcision as dangerous, much less fatal.

The circumcision of the Jews differs little from circumcision in simple or complicated phymosis. The child is bandaged tolerably tightly from the symphysis to the shoulder, and from the middle of the thighs to the ankles, and then laid upon the lap of its godfather, seated. The operator, with the thumb and forefinger of the left hand, lays hold of the prepuce, and brings the portion to be removed within the

cleft, two lines broad, of a silver instrument similar to a mouth spatula, and when the member is brought in a perpendicular position, seizes the inclosed foreskin, and with a single stroke of a double-edged bistoury, cuts it off close to the plate. With the thumb nail expressly sharpened for this operation, he tears as quickly as possible the inner membrane of the foreskin up to the corona. The bleeding is stopped by cold water.

A primitive and unrefined custom of spirting red wine several times from the mouth, and sucking up the blood at successive draughts, is now only kept up by a few very orthodox common practitioners. The hæmorrhage having ceased, the wound is by some operators sprinkled with Sem. Lycopod; by others, the glans and the cut surface are covered with a strip of linen, spread with fresh butter.

Under Prof. Pitha, as house-surgeon, I had a patient, 18 years old, on whom circumcision had been performed, whose wound healed in eight days. With children this is accomplished in from 24 to 36 hours, from the fact that the external and moral agencies, so powerful in adults to disturb the healing process, are with them wholly wanting. If in Dr. L.'s practice of 16 years, there happened six cases in which the operation resulted fatally, it may be well asked if it were due to the gravity of the operation, or the unskillfulness of the operator. Dr. L. will not dare to instance blood-letting as an operation dangerous to life, because here and there an unskillfully performed venesection is followed by fatal results. How lame, therefore, are all such arguments of Dr. L., is evident enough to every practical physician! As an invincible fact, moreover, stands out above all, the extremely favorable statistics of mortality of Jewish children in the first year of their life. According to E. Glatters' Reports, (the life-chances of those of the Jewish and Christian profession contrasted. Biostalistic Studies. Wetzler, 1856,) during a period of 32 years' observation in Wieselburger County, Hungary, there died of a thousand, in the first year of their life, of Jews, 44.1; of Germans, 123; Hungarians, 167; and Croats, 146.9. From these data, it appears clearly evident that circumcision exercises on the one hand no injurious influence on the ratio of mortality of Jewish children during the first year of their life; so on the other, that it is greatly in its favor, that it not only gives no predisposition to those pernicious diseases of later life, which, even when concealed, so deeply affect the human organism, but should be recommended authoritatively as a preservative against them. Upon this point we can adduce, among

others, the statistical reports of Prof. Hutchison, (Med. Times and Gazette, 1856.)

In an article in the Berlin Allgem. Medic. Central Zeitung, entitled "Upon the Influence of Circumcision in the Contraction of Syphilis," are these words. The idea has very often been expressed that circumcision, inasmuch as it brings about an anatomical change in the male genital organ, by converting the membrane covering the glans into a resisting epithelial coat, the disposition to contract syphilis is lessened.

Every physician having any Jewish patients, knows that syphilitic ulcers rarely occur among them—that for the most part they run their course more rapidly, and are seldom followed by secondary phenomena. Especially has he, with them, from the nature of the case, nothing to do with that dangerous chancre which, located on the inner surface of the foreskin and removed from view and treatment, easily increases, eats its way deeply in, becomes indurated, and is followed by the most unhappy secondary and tertiary symptoms.

Hutchison has now, for the first time, so far as we know, made the attempt to give in precise terms the comparative frequency of syphilis among Jews and Christians. And by statistics, to prove the correctness of what hitherto has been accepted à priori.

Dr. H. is physician to a London Hospital, which, connected with a Dispensary, is located in a portion of the city inhabited for the most part by Jews. Frequent application is made by them for Hospital and Dispensary aid.

The total number treated for syphilis in 1855, amounted to 330—of whom 272 were Christians, and 59 Jews. Of the former, 107 for gonorrhœa, 165 for syphilis. Of the latter, 47 for gonorrhœa, and 11 for syphilis, which gives the proportion of gonorrhœa to syphilis among Christians, as 0.6:1, and among Jews 4.3:1. The proportionally great frequency of gonorrhœa among Jewish patients (they furnish almost the half of all the collected cases,) shows that the difference in the total of syphilitic patients is not to be accounted for by a greater reserve on the part of Jews to apply for medical aid.

During the above period, there were treated in the same Hospital, 252 children under 5 years of age, 179 of whom were Christians, and 73 Jews. Of the former 27, and of the latter 3, were unaffected with congenital syphilis. The number of females treated for venereal diseases amounted to 97, of whom 92 were Christians, and 5 Jews. Of the former, 61 were treated for syphilitic ulcers. Two-thirds of the Christian patients were married, and in all probability received

the infection from their husbands. The proportional frequency, so favorable in the case of Jewish women, can partly arise from their greater morality; from their domestic life being based more upon ethical principles, but more especially from this, that the diseased Jewish husband is not in so favorable a condition to conceal and communicate his disease.

In view of this fact, the old Mosaic rite receives a confirmation, perhaps stronger and more binding upon our age than that in which the Legislator lived; and, from a hygienic point of view, the general introduction of circumcision would be founded more in reason than its often talked of abolition.

Embalming as carried on by the Indians of America. From the French of M. ALVARO REYNOSO. For the MONTHLY.

The methods employed by the Indians to preserve the dead can be considered as of three kinds; the bodies were either skinned and then *stuffed*, or they were *embalmed*, or simply *dried*.

1. Some of the North American tribes commenced the operation by skillfully removing the skin from the corpse, having made an incision the whole length of the back. They picked the flesh off from the bones with care, without touching the ligaments, so that the skeleton might remain entire. The bones, after they had been dried for some time, were again placed in the skin, which had been carefully prepared and softened, and the latter being filled up with fine sand, was sewed up again. (Lafitteau, *Mœurs des Sauvages Americains*, II, 389.) Las Casas says, that in a village of the Province called Cali, there existed a very tall wooden house, covered with straw, having one door and four windows. In the interior of this house, at a certain distance from the ground, many human beings were arranged after a certain order, on a large board which extends across. To prepare these bodies they had simply filled the skin with ashes, and added figures modelled in wax, with their features so well reproduced that one could have believed at first sight that the bodies were alive. (Las Casas. Apologetica Historia de las Indias, 872.) These two methods are rather processes of flaying than embalming.

2. In Peru, an odoriferous wood was burned before the idols. When the bark of this tree was removed, there flowed a liquid with such a penetrating odor, that it soon became insupportable. Bodies varnished with this liquid, and in which a certain quantity was introduc-

ed by the mouth, never putrified. The custom prevailed of placing in the temples some bodies thus embalmed, or skins that had been submitted to the same process.

3. The method next to be described, was based on the desiccation of the bodies, which was effected not by means of solar heat, but of fire. Las Casas, in reporting the interview of Vasco Nunez with the King of Comagra, in Darien, tells us that in the palace of that king there was a large room containing many dried bodies, which were suspended by means of cotton cords, and covered with rich coverings also of cotton, interlaced with trinkets of gold, pearls and other stones regarded as precious in that tribe. These were the bodies of ancestors, who were considered as the tutelary divinities of the fire-side. (Las Casas, Historia general de las Indias, 111, 146.) Whilst in other countries, says Las Casas, bodies are preserved from putrefaction by balsams and other aromatic substances, the Indians obtain the same results by means of desiccation with fire. He describes the operation thus: after having wept over the deceased, they envelope the body in cotton coverings, which are bound with cords; then they place it on a grate over a slow fire, "to drive off all the moisture contained in the body," and in this way complete desiccation is accomplished. The grates were made of thick canes. In the Kingdom of Popayan, instead of placing the dead body on a grate, it was suspended in a hammock over the fire during the time required for the desiccation. These quotations have never been cited before, as Las Casas' manuscripts have not been widely circulated, and those persons who possess them have not paid attention to these details.

We might further cite the testimony of Oviedo, *(Relacion sumaria de la historia natural de Indias,)* and of Lopez de Gomara, *(Historia de las Indias,)* who describe the proceedings in desiccation, more or less perfectly, and on the whole similar to those we have learned from Las Casas.

I believe that these processes could be applied, if it should be necessary to preserve a large number of dead bodies without enbalming them. They could be dried rapidly by placing them in a heated chamber, a current of warm air being driven over them by means of a ventilator.

M. Gay, in his report, defends Gonzalo Pizarro from the charge of having profaned the sanctity of the tomb of the Inca Viracocha. It affords me pleasure to cite an authentic witness in support of this opinion. In the XLII volume of a collection of unedited documents on the History of America, made by Munoz, and preserved among the

manuscripts of the Academy of History, at Madrid, there is a report presented by Ondegardo, on the tributes which the Indians paid to their sovereigns. Ondegardo says, that at Cuzco was found the body of the first Lord of Cuzco, whom every one considered as the first who had conquered and taken possession of this country. This body was embalmed and in a state of perfect preservation. By counting the number of Incas who had succeeded him until the arrival of the Spaniards, the body was estimated at 300 years old. Ondegardo also says, that he found the body of the Inca Yupangi embalmed, and at his side the symbols which made known his valor, as well as the festivals and religious ceremonies of his day.

Before closing this note, I wish to make two general observations on natural mummies. I believe that, up to this time, attention has been too exclusively directed to the *physical* properties of the soil in which natural mummies have been found, and that we have neglected to analyze *chemically* the soil with the design of knowing if there existed in it any salts capable of preventing putrefaction, which could penetrate the dead body and preserve it. Further, and I dare scarcely hazard the conjecture, I believe if certain bodies resist putrefaction better than others, although they may be found exposed to the same conditions, that the difference can be explained, either by the regimen that has been observed through life, or by the medicaments which have been employed, and above all, because they may have been more readily desiccated. I could cite, in support of this opinion, many facts, but I prefer to bring forward but one, which, on account of its historic interest, unites the advantage of authority above suspicion, to ready verification. The body of Charles V., which had not been embalmed, still remains in the Pantheon of the Spanish Kings, in the Escurial, and is in better preservation than all the rest whom they have endeavored to preserve through divers plans. Under Philip IV., in 1654, eighty-six years after the death of the emperor, the body was exposed to the public, and the people attested its preservation. Another contemporary relates that independent of the nose, the whole body, even the beard, was so well preserved that the physiognomy of the king was easily recognized. The flesh dry, and the body seemed thinner than natural; and a circumstance worthy of remark was noticed, that the bier of wood on which the body lay was entirely destroyed. Last year it was again attested, in the presence of many respectable persons, that the body of the emperor was still in a state of perfect preservation.

S.

Report of a case of Cancer of the Œsophagus, occurring in Bellevue Hospital. By J. J. CAMPBELL, M.D., late House Surgeon.

D. G., aged 57, a native of Scotland, good constitution; a baker by occupation; was admitted into the hospital August 28th, and gave the following history: He says he has always enjoyed good health, with the exception of a slight cough at times, and that he has never been either fleshy or strong. He knows of no hereditary predisposition. Never has indulged much in liquor, although he was in the habit of taking a little every day, for many years. Never has swallowed any corrosive material, and knows of no cause for the present difficulty. About four months ago he began to suffer from difficulty in swallowing solid food, and this difficulty rapidly increased until two weeks since, when he was unable to get anything into his stomach. He says all his food and drink stops at a point opposite the lower border of the sternum. From two to three minutes after swallowing solids or fluids, they regurgitate mixed with ropy mucus, which is sometimes tinged with blood. He also attempts to vomit at these times. When admitted he was very weak, much emaciated, and was unable to get any nourishment into his stomach. A flexible probang was introduced into his œsophagus, and stopped opposite the lower margin of the sternum. There was no evidence of sacculation. On auscultation, no aneurism could be detected. In the apex of the right lung a cavity was found.

As he was unable to get any nourishment by his stomach, beef tea injections were employed. He could not retain more than three pints of it a day. Brandy was added to the beef tea after a few days.

He continued growing weaker and weaker, until he died, Sept. 17th. No marked suffering or struggling before death.

Autopsy nine hours after death. The skin presented the same pale or sallow hue that it did previous to death. The left lung was perfectly healthy, but the right one was firmly adherent to the walls of the chest, and contained hardened tubercles and a tubercular cavity about the size of a hen's egg. The heart and aorta were quite healthy. The femoral and radial arteries were ossified in patches. The œsophagus, stomach and duodenum were removed and examined. The lower three inches of the œsophagus formed a solid mass of scirrhous hardness. The stomach was small, and could not contain more than a pint of fluid. Its pyloric orifice was very well marked by a hard thickened ring. On placing the duodenum on a Croton jet, and the water allowed to flow in slowly, the stomach became distended, but none of the water escaped. The œsophagus was then placed on the

jet, but none of the water would flow through. The water was then let on with full force, and the œsophagus bursted longitudinally, commencing at the diseased point and extending two inches upwards. The diseased mass was perfectly circular, and about three-quarters of an inch in diameter. On cutting into it, its outer surface cut like a fibrous tumor, and in its interior it was soft, having a dirty grayish appearance. It was pervious to a point two and a half inches above the cardiac orifice of the stomach. The œsophagus was not dilated more than usual above the point of disease.

The prostate gland was normal in size, and perfectly healthy. All the organs were healthy, but quite anæmic.

Report of a case of Castration for Tubercular Disease of the Testicle, occurring in Bellevue Hospital, in the service of Dr. Jas. R. Wood, Visiting Surgeon. By J. M. FARRINGTON, M. D., House Surgeon.

Charles G., aged 26, a native of Scotland, a farmer; single, of delicate constitution, was admitted in hospital Nov. 14th, 1857, with tubercular disease of the right testis.

When a child he had scarlet fever, and at the age of 16 the small pox, from each of which he recovered, without the occurrence of any unpleasant sequelæ, excepting that an inflammatory affection of the left eye followed the attack of variola. He has been a victim to the practice of masturbation since the age of 12. He contracted a chancre when but 14, which was followed by constitutional syphilis.

A few months ago the right testicle began to enlarge slowly, and continued to do so until six weeks since, when an abscess formed in the organs. This opened spontaneously in 9 or 10 days, and discharged about an ounce and a half of pus. Ever since it opened there has been going on a gradually increased protrusion of a fungoid substance, which has now attained the size of a moderately large plum. He has a cough occasionally, but it is not persistent. He has none at present, and on examination of the chest no physical signs of disease were detected.

He is pale, somewhat emaciated, small in size and stature, and never weighed over 112 lbs.

His mental condition is dull, and his habit slovenly. The circulation is sluggish, the pulse 60.

On the 21st, the diseased testicle was removed by Dr. Wood. An

elliptical incision extending from the external abdominal ring to the lower part of the scrotum, and including the diseased and protruded mass, was made. The dissection was then proceeded with; the adhesions between the gland and scrotum were firm; the organ was everted, the spermatic cord divided, and its vessels secured by ligatures.

A few hours after, the edges of the wound were approximated by the silk interrupted sutures, but the inflammation and tumefaction of the parts were so great that most of the sutures became detached by ulceration within 36 hours. They were inserted again, and the third time with the like result, and it was then deemed useless to employ them further until the inflammation and swelling should have subsided.

A lotion of acetate of lead and opium was kept upon the parts. The ligature attached to the spermatic artery came away on the 17th day. The inflammation subsided, the suppuration diminished, and the wound began to fill up finely by granulation, so that no further attempt was made to bring the edges together by sutures.

Granulation and cicatrization proceeded finely, and the patient rapidly improved in appearance, in every respect; so that, as Dr. Wood once remarked of a similar case, "he was an *altered* man indeed." He gained flesh and color, and his mental condition was unquestionably improved.

He was discharged Dec. 31st, 40 days after the operation, entirely cured.

REVIEWS AND BIBLIOGRAPHY.

A Manual of Medical Diagnosis, being an Analysis of the Signs and Symptoms of Disease. By A. N. BARCLAY, M.D., Cantab et Ed. Reprint. Philadelphia: Blanchard & Lea, 1858.

The author, in his Preface, apologizes for adding another to the many Manuals already in the hands of students, but finds a sufficient reason for his publication, in the wish which has been expressed in his hearing, for a guide to the systematic investigation of cases in the wards of the hospital.

The obvious utility of works of this character makes it a matter of surprise that they are very rare. A reviewer of Rostan's work in 1827, called attention to the fact, that but one work on this subject, Diagnosis, existed in the English language, at the time of his writing. In the course of kindred investigations, we have had occasion to look over book-shelves, and through many volumes of periodical literature, in search of treatises and essays on Semeiology and the art of Diagno-

sis; and note it here as a fact of bibliographical interest, that hardly any subject connected with practical medicine, directly or remotely, has been so imperfectly treated in the records of the last century's contributions to the literature of our science. When we have enumerated the works of Dreyssig, Küttner, Friedreich, Wenzel, Richter, Nasse, Siebert, Sucknow, Schiel, Blumenthal, Rostan, Piorry, Dubois d'Amiens, Valleix, Chomel, Bouchut, Oppolzer, Williams, Marshall Hall, Copland, Bartlett, Reynolds, Barclay, and a new translation of Aretæus, very few special treatises, or papers, on Diagnosis remain. It may be added, as not without interest, that the first 60 vols. of the Archives Générales do not contain a single article on Diagnosis; that the Edinburgh Med. Essays, 6 vols. [1731–1771]; Essays Physical and Literary, 3 vols. [1754–65]; Medical Commentaries, 20 vols. [1773–1795]; Annals of Med., 8 vols. [1796–1804]; Edinburgh Med. and Surgical Journal from 1804, contain only five articles on the general subject of diagnosis, of any pretensions; two of which are by Dr. Abercrombie and Dr. Marshall Hall. The 57 vols. of the Med. Chir. Journal, and its successor, Medico-Chirurgical Review, contain only two—one a review of Rostan's work, and one of Dr. M. Hall's. In 48 numbers of Forbes' British and Foreign Review there are but 3 or 4 articles. Until Schill's was translated, no work on Semeiology existed in our vernacular. From which facts, gleaned from the literature of Germany, France, England and Scotland, it may be inferred that medicine has heretofore been taught in the scholastic and scientific method, beginning with the organic changes, and working out towards external manifestations or symptoms. To the practitioner, disease presents itself as an outside fact. It is first to be seen and heard. The reasoning comes afterwards, in order of psychological phenomena. And it is from this direction that Dr. Barclay presents disease to us.

Books like the one before us, and the little Manual lately published by the London Medical Society of Observation, indicate the growth of Clinical Medicine, and have grown out of its necessities. Dr. Griffin, in his self-defending report of the very unlucky case of Mr. Augustus Stafford, makes the following *naive* remark: "If every man standing at the bedside of a patient, could have his reasoning on the case illuminated by the light of a post mortem examination, I believe there are numbers of cases in which treatment would very seriously be modified." Between the bedside of the sick and the post mortem table rushes a dark current, which can best be bridged over when diagnosis has laid the foundation stones.

The succession of mental processes which engage the medical bibliopholist when he is turning over Lebert's folio plates, is reversed when, as a thoughtful, conscientious, sympathetic therapeutist, he stands by the bedside of his sick friend. At home, in his study, with necroscopsic scalpel in hand, he reasons from a localized cause out to general and diffused effects; and so does he when he follows the teachings of the schools. And this, at some time or other, he must needs do; but in every case of disease under his own treatment, he must reverse the glass thus: John Doe stands for dulness under the right clavicle—lung tissue in the field of the miscroscope—tubercular *stuff*—a cavity in the right apex, partly lined with pyogenic membrane—large greasy liver, &c. &c. Richard Roe represents a cheerful, pink and white young gentleman, with fine hair and glistening eyes, who tries to go about his business, but wonders why his hands are hot, and why he coughs, and why he wakes up before daylight, cold and sweating. Problem, to reconcile John and Richard.

We find no science resembling so much that of medicine as Geology. The phenomena, geological and pathological, are apprehended from the same two divergent directions; we desire only to point out one set of common facts as examples. The Palisades, it is said, indicate an eructation from the inflamed belly of mother earth in the days of her youth. That is one way of commencing an investigation; or, beginning at the other end, you state it thus: the central heat-forces in process of time, with alternating radiations, must produce cracks and bulgings, palisades, old men of the mountains, &c. In the same way we say, paralysis indicates destruction of nerve-tissue, more or less, complete and permanent. The escape of blood from the pia mater meshes produces anæsthesia and spasm, loss of appetite, &c. The latter form of presenting the fact is more proper; to the clinical student the former is the more presently useful and handy. We are ready to acknowledge the necessity of books which so turn the matter round, notwithstanding the objections which Louis has been said to offer against them; but even he would only say of diagnosis what he said of pathological anatomy, "quelle ne me parait pas devoir constituer actuellement une science à part, pas plus que le pronostic." [Mem. de la Soc. Med. d'Observation, v. 1, p. 19.] To our mind, the necessity of books having the aim which our author takes is no longer matter of dispute. Something might be said concerning the natural and necessary limitations to their usefulness; for it is not to be doubted that one may have at hand all the means and possess all the knowledge for apprehending and interpreting the distinctive signs of a spe-

cific change of structure, and yet fail to read aright its relations, and so go wrong in matters of diagnosis and therapeutics. The symptoms are but notes and phrases, the whole disease is a dirge. The possession of all the tools of one's trade, and facility of manipulation, do not without fail ensure a good workman. One may be a true draughtsman, or a choice colorist, and yet having no personal thought or feeling that wants its way, be no artist.

Most men study some one or more branches of medicine, and drop them before they have learned the usefulness of their secrets, as they drop Greek and German before they have gone far enough to feel the nice beauties of their literature.

We may be able to trace the Vidian nerve, collect a bowl of starch from degenerate brains, a modicum of sugar from the liver, and go straight to the fourth ventricle to get it; be able to demonstrate crystals of excretine and hæmatoidine; keen to detect all or any one of Siebert's six groups of physiognomical *rugæ;* follow Pfaff and Ahrens in determining the electric relations of a patient to that of the earth and atmosphere; we may be alertly alive to the latest bin-auricular stethoscopic phenomena; may see mysteries written in urinals, and wonders, not to be told of, at the little ends of specula, and after all be utterly unable to see the good of them, and incompetent to turn the incomplete knowledge to any substantial profit. No fact stands alone in the world; and pulmonary crepitus, urinary albumen, muscular spasm with all the rest, have past and future relations, which must be known to make physical signs of any account.

It may not be altogether unprofitable to follow this point to the end. The knowledge of a case of disease, complete and *exhaustive*, as the phrase is, is reached by four stages:

First, the examination of the patient as to his history, &c.
Second, " " his subjective symptoms.
Third, " " objective phenomena.
Fourth, the interpretation of the facts obtained by the preceding process.

These proceedings are quite independent of each other, and widely different in character, so that the ability of carrying them all on equally well is rarely possessed by any one. It is apparent that,

The *first* demands the frank, pleasant manner which disarms fear and wins confidence, associated with keenness to strike a scent, and perseverance to follow a trail.

The *second* requires a patient, minute questioner, but skeptical listener; one acquainted with the laws of evidence, who weighs, filters, and winnows.

The *third* puts in requisition great knowledge, self-cultivation, keenness, patience, accuracy, self-confidence, tempered with questioning doubt; facility in the use of all the armamenta, spirometer, pleximeter, specula, stethoscope, microscope, chemical reagents, &c.; persistence in accomplishing measurements, palpations, digital examinations, inspection of excreta, &c.

The *fourth*, which is the crowning fact that answers the *cui bono* inquisitor, demands first of all brains, and brains of particular mould and finish—brains which, large enough to be self-nourishing, are not too large to grow, and are impressible by fresh experiences.

Every one can find examples of those who limit themselves to one or two of these processes of investigation; and most men start professional life with an exaggerated estimate of and excessive trust in the diagnostic means enumerated in class third. And this is all very well. It is the young man's business to understand and use these means. If he don't do it when he is young, be sure he will never do it at all. But some good friends ought privately to say to him, "Galen, my boy, brandishing forceps is not an essential element in obstetric processes; children are now and then born alive without the assistance of that instrument, among people of a low grade of civilization; now, put up your weapon. And it is not at all necessary that you should be forever foggy over your microscope; get all the facts you can, by whatever way is best, and then sit down, and do a little hard thinking, or else you will have used your eyes and ears and battery of instruments in vain."

It has been said that one may be quite successful if he does not push his investigations beyond the first step. Surely, friendliness and frankness should look out of a doctor's eyes; and if the valetudinarian needs nothing more, it's all the better for the valetudinarian. But good nature and pleasant manners are only vehicles by which a doctor gets about. The business of his life demands sterner stuff.

* * * * * * * * * * * *

Dr. Barclay, while serving as Medical Register of St. George's Hospital, (to our mind one of the most progressive of the London schools,) had opportunity to qualify himself for the task which he has undertaken in the work whose title stands at the head of this article. In this office twelve thousand cases of disease passed under his notice for detailed registration and classification.

In the form of a schedule he gives, as the result of his experience, "the outlines," as follows, "of the particulars which a clinical clerk ought to attempt to enumerate in the history of each case which he records," thus:

Address, name, age, civil state, occupation. In females, number of children, date of last pregnancy, menstrual function.

History:

a. Of present attack.

b. Of previous illness.

Present state:

1. General symptoms:
   a. Skin, as to heat and dryness.
   b. Pulse, as to frequency, force, and fullness.
   c. Tongue, as to coating and moisture.
   d. Bowels and Urine.
   e. Appetite and thirst.
2. Appearance:
   a. Size.
   b. Aspect and expression.
   c. Color.
3. Position and posture:
   a. In bed.
   b. Out of bed—gait and manner.
4. Sensations—survey of regions and organs.
   1. Innervation:
      a. Brains.
      b. Nerves.
   2. Respiration.
   3. Circulation:
      a. Heart.
      b. Blood vessels.
   4. Digestion:
      a. Assimilation.
      b. Excretion—character of stools—analysis of urine.

A reviewer of Dr. Siebert's work (Forbes's Rev., Oct. 1845) quotes the following heads of a masterly detail of the facts in a case of heart disease. Firstly, the man's medical biography; the commencement of the disease, then its course hitherto, with the patient's present condition, under the subdivisions of external appearances, symptoms implicating the circulation, respiration, digestion, and feelings of the patient. The whole phenomena of the disease being ascertained, they are next studied, and their relative value estimated. The anatomical signs of hypertrophy are compared with the functional disturbance in the heart's action, and these with the other symptoms, the objective and subjective, the local and positive, the idiopathic and sympathetic, deuteropa-

thic and tritopathic, &c. The second step is the arrangement of the symptoms into a known form of disease. The last step is the consideration of the individuality of the patient, the amount of health remaining to him, and the relation of the disease to the weather, and the epidemic "genius."

The following extract from Suckow's *Grundriss der Speciellen Semiotik, Jena*, 1838—and properly enough styled by a reviewer of the work as Babylonish—is an example of diagnostic refining, which is anything but clarifying: "*White tongue*—indicates catarrh, aphthæ, sore of the mouth, tobacco smoking, ptyalism, gastricism, scrofula, rickets, defective nutrition, long confinement, insanity, induration of the abdominal viscera, diseases of the heart, peritonitis, hepatitis, splenitis, ulcerated intestine, induration of the lungs, gastric, rheumatic, catarrhal, nervous, mucous fever, hæmorrhoids, hypochondriasis, phrenitis, meningitis, inflammation of the chest, ague with gastric complication, dyspepsia, phthisis."

Clearly, to Suckow might rightfully be applied the epithet which Coleridge mentions as well deserved by Kant, "*Der alleszermalmende*" —the all-to-nothing crushing—for Suckow goes on in that way to the end of the chapter.

In the introduction, the author declares himself in a few well-placed sketches in outline, by which we can judge of his views and standpoints. Though the generalities are sufficiently commonplace, they effect us pleasantly, and prepare us to think well of the writer. Thus the "ultimate object of study in all departments of medicine is the relief of the patient by the successful treatment of disease." The theory of disease combines, by the aid of experience, the perversion of function with the change of structure; * * * "but it also teaches that there are other and more hidden elements of disease * * which have yet evaded our most diligent search." Diagnosis is the hyphen which unites semeiology and nosology; and the only legitimate introduction to the wards of a hospital.

The following opinions we hold to be of such import that we transcribe paragraphs somewhat too lengthy for this part of our subject:

"If it were true that the symptoms by which a disease is recognized were analogous in all cases, it would be enough that the student should commit to memory the summary contained in systematic treatises, when he would be at once in a condition to pronounce an opinion upon any case put before him. But this is far from being the case. The idiosyncrasy of the individual, including in this term all the differences exhibited by various persons in their susceptibility to the influence of the same noxious substance or emanation; and not less than

this, the various power of the causes of the disease, which, though unproved, and perhaps incapable of proof, we cannot deny, exerting an influence now more potent, now weaker; the combination of these two circumstances leads to an almost endless variety in the outward manifestations of their operation on the human frame."

This we hold to be sound doctrine, and one to be iterated, notwithstanding Maclise's grotesque criticism of similar opinions, thus: "for of a certainty this can't be that, since that is dappled, whereas this is striped; nor can their species be the same and one, though they be puppies of the self-same litter."

According to the true principle of diagnosis, having associated certain phenomena observed through life with particular lesions found after death, we learn to group symptoms together, and apply to them a scheme which shall in some way account for their existence. We gather evidence in accordance with the best method we have at command, and so conduct our investigation as to lead ourselves right, and impress our patients with confidence as to our ability to find the right track and keep it. We must have accurate knowledge of the theory of disease; must not attach too much value to signs regarded as pathognomonic; must not trust too much to one symptom, or one group, and be ready to discard an hypothesis founded in insufficient premises, whenever new symptoms shall come to light on subsequent examinations, because diseased action in the body is often very complex, and the phenomena present may not be all reducible to the results of one form of disease, or a morbid condition of one set of organs. Before all things, the general strength of the patient must be borne in mind. It is not enough to know the disease; but, as Chomel always says in his introductory, the degree of resistance which the patient interposes is the main thing in determining the result. To discover inflammation in every sickness, and then proceed to fight inflammation, in Don Quixotte style, is ridiculous in the first party, and lamentable for the second. Such doctors spend their lives in treadmills, if they don't get carried round on windmills.

In chapter 1, Dr. Barclay treats of the method of diagnosis, in the course of which he gives a plan which he has found serviceable in making available notes of a large number of cases. Having said that the student, when introduced to the bedside of the patient, should carry in his mind a certain definite course of inquiry, he gives advice so useful that we must save space for it.

"The student will do well to commit to writing the result of his inquiries. There is no means nearly so successful in giving system and correctness to his investigation; in no other way can he acquire the

habit of observing all the phenomena of any given case, or tracing their bearing on each other; and nothing will so effectually teach him to mark correctly, and estimate justly, each successive fact elicited by his own inquiries, or volunteered by the patient. From the whole evidence, thus faithfully committed to writing, can he alone hope to form a correct diagnosis. His written description ought to be a full and adequate account of all that he sees, hears, feels, or even smells, and must never embody any conclusions he has formed from them until the whole inquiry has terminated. Thus, to take a prominent example: In examining the lungs, however distinct he may fancy the evidence of a cavity to be, he ought never to put down in his notes "cavernous rales," or "cavernous breathing," but what he actually hears—gurgling sounds, loud or very loud, blowing, expiratory breath sound, &c.; everything, in fact, just as it is heard. As he proceeds, it is quite possible that other signs or symptoms may be observed, so inconsistent with the hypothesis that it would be quite unwarrantable to assume the existence of a cavity—a conclusion which ought only to be formed from the coincidence of several other phenomena."—p. 28.

No thoughtless wastefulness is more improvident than that of which physicians of large but *unrecorded* experience may justly be accused. Had all the observed facts, or only the noteworthy facts, been made matters of record, the rebuke of the author of the Advancement of Learning would not have been deserved at the time when it was written. But *væ vobis* it is just as true now as ever it was. When Bacon said, "Medicine is a science which has been more professed than labored, and yet more labored than advanced, the labor having been rather in circle than progression," he said a true thing which will never cease to be true till this waste of unnoted fact and commentary has ceased.

Chapter II treats of the *duration and sequence of phenomena*, and gives opportunity for the following remarks concerning the valuation of pain as a sign of disease:

The important bearing of the duration of pain will fall especially under consideration with reference to hysteria and neuralgia. Here it may be observed, how impossible it is, from the description of the patient, to form any idea of the exact amount of pain and suffering, or to institute any comparison between the expression of it as employed by different individuals. One will talk composedly during a severe operation; another looks pale and haggard, and seems to be in great pain, perhaps really does suffer much from a mere nervous affection, which exists chiefly in the imagination, and is principally maintained by the attention being continually directed to it. Here, then, is the inconsistency, that a very unimportant distraction serves to withdraw the attention, and thereby removes all recollection of its existence and every indication of its continuance. A third person suffers severely from paroxysms of pain, which no amount of pre-occupation can prevent, no distraction during its continuance can suspend; yet, in this

case, there may be no structural change to account for the presence of pain. The power of distracting the attention is often the only distinction between that which is unimportant and transitory, and that which is of grave import and exceedingly untractable, until its duration and recurrence, and the exhaustion it produces, point out its reality."—Page 33.

Concerning the succession of symptoms, and the doctrine of self-limited diseases, our author remarks: [pp. 33 and 34]

"A certain amount of caution is necessary in adopting the patient's description of the order of sequence of symptoms. It is remarkable how, in slowly advancing maladies, nature accommodates herself so completely to immense alterations in structure, that, until some unusual event occurs, the patient is utterly unconscious of any deviation from health; or, it may be, there is only a sense of malaise, without the possibility of tracing this feeling to its cause, or of naming any single symptom which has attended it. Suddenly some change occurs, of which the patient becomes cognizant; and then other sensations, which previously existed, take form and shape in his mind, and consequently find place in his description, after that which is, in reality, their effect and not their cause."

Again, so intimate are the relations maintained between all parts of the body, that it may not be in the very locality in which disease has commenced that symptoms of its presence first arise; and hence it sometimes occurs that the first feeling of illness may not directly point to its true seat. This must be corrected by knowledge of the theory of disease, and the various symptoms by which it is accompanied.

With these qualifications, the first real deviation from health is of much value in leading us back to the true seat of disease. Most diseases have a certain established course, which, either in broad or general outline, or even in minor detail, is followed by all the examples coming under observation; and although we cannot prescribe the exact limits of these sequences, either in days or weeks, in the majority of instances, yet there are, in all, general periods of greater or less duration, during which certain phenomena must present themselves, or else our diagnosis has been utterly at fault. This fact forms one of the elements of prognosis, and points out its association with a just discrimination of the nature of a malady in the first instance.

The general condition of the patient is the next subject considered—Chap. III. The phenomena to be observed are either objective or subjective; and are divided into the four groups corresponding with the groups under the head of "*present state*," given in the schedule.

This chapter includes the consideration of those symptoms and signs

which are chiefly the subjects of observation in ordinary practice, the temperature, pulse, tongue, state of the bowels, urine, &c. Under the head of temperature nothing occurs worthy of note; but on the other hand, omissions of some consequence. For example, no allusion is made to the class of facts for which Bernard and Brown-Séquard have furnished explanations by nerve sections, and Wharton Jones by ratiocination applied to malaria; we mean local and limited elevations, and depressions of temperature. We remember a lecturer who was accustomed to insist much on the necessity of estimating rightly the signs furnished by hot palms, hot soles, &c., as distinguished from heat of surfaces along the extensors. A hot ear indicates paralysis of the capillaries, secondary to interruption of the nerve current in a cervical ramuscule of the sympathetic, says Brown-Séquard. At all events, it indicates a change among the ultimate molecules, concerned in the processes of nutrition. Local sweating is also a symptom to be marked, and, artificially produced by gutta percha lamina, is valuable as a remedial agent.

Pulse—how to measure it? and what it means? are questions in some cases easily answered. In cardiac diseases, in hydrocephalus, and the late stages of typhoid fever, there can be no doubt as to the story it tells; but there are many cases in which it makes a plain revelation only to the *tactus eruditus*.

One striking symptom our author finishes by two lines, thus: "Irregularity of pulse has very important bearings upon special forms of disease, but is of less consequence as a symptom of the general condition of the patient." Now we believe the fact here to be just reversed. Who has not felt intermittent pulse in influenza, and in all the forms of fever? and in that well-ascertained, if not well-defined, condition which Travers used to call "*constitutional irritation*," (we trust our young readers will excuse our using a term now so much out of date, inasmuch as it was thought in our best days to express a fact,) and even in those who have considered themselves as not unhealthy, and certainly free from cardiac valvular disease.

What is said concerning the physiognomy of disease should be especially commended to those who, in the hurry of the clinique or dispensary, must find means of knowing the whole affair with half an eye. There seems to be here a propriety in Bartlett's division into nosological diagnosis and therapeutical diagnosis.

We pass over remarks concerning the information derived from the aspect of the tongue, and also the scanty observations concerning the state of the bowels and urine, calling attention only to Piorry's pecu-

liar views concerning the former, and Oppolzer's and Marcet's minute attention to the latter class of facts; and particularly to the review of the Essay of M. M. Delon and Reeve, contained in the February number of this Journal.

The chapter on Special Indications closes (p. 43–46) with a tabular arrangement of compact statements concerning the stools, urine, aspect, alterations of color, position in bed, gait, &c., to which we refer our readers.

Having spoken of general signs and symptoms, our author next proceeds to take up special diseases, and diseases of special organs. We see on which side he stands in relation to a *questio vexata* of the time by the following sentence: "*there is a large class in which local disorder, as manifested by symptoms belonging to particular organs, is only secondary and subsidiary to the general disease.*"

Dr. B. does not insist much on the distinction between typhoid and typhus fevers, although several of the London writers have frankly changed their ground to correspond with Dr. Jenner's views. By none has it been done more promptly or more gracefully, than by Dr. Watson. We note also the following Anti-Clutterbuck sentiment, not because it would be particularly noteworthy on this side, but as an index to Dr. Barclay's Philosophy: "The prominent feature of the disease (continued fever) must never be lost sight of, that it is not *inflammatory;* it is a condition of asthenic pyrexia. In the head, we have delirium, insomnia, unconsciousness, coma; that these are not due to inflammation is proved by the history of the case."

In the lungs, congestion almost always comes on, more or less, from position, but this is not true pneumonia, &c.; p. 54.

Again, "*Remittent Fever* is not generally believed to be only typhus, as modified by atmospheric influences, and the condition of the nervous and sanguiferous systems of Europeans residing in tropical latitudes. The same analogy holds with reference to the only fever of this type ever seen among ourselves, infantile remittent, and there is nothing to show that infantile remittent may not arise even from the infection of typhus." The last is so cautious a statement, that it ought not to be exposed to criticism; we would then only reply, that, 1st, Infantile Remittent is an extremely prevalent disease in this latitude and longitude. 2d, That it is prevalent when Typhus is not about; and 3d, That Typhoid, which Dr. B. would consider a relative of Typhus, but which we do not, any more than scarlatina is of measles, is not infectious at all, to the best of our belief.

Still further on, when speaking of Influenza, our author says: "In

an enfeebled constitution, the least disturbance may provoke symptoms of general derangement, with fever of an asthenic type, closely allied to influenza; exactly as more severe disease may in the same constitutions cause typhoid symptoms or symptoms resembling typhus."

We detect the ambiguity of the last sentence, but only object to the sentiment, derived from the reading typhoid symptoms to be symptoms resembling typhus. If that is the author's meaning, we should join issue with him on the whole subject of specific disease and specific causes of disease. We believe influenza to be the result of a specific morbific impression—thermal, electric, hygrometric, or something else—specific at all events.

So too, in our view, is typhoid quite another thing from typhus—*ab initio* another thing. It is quite possible that a man with a common cold may get influenza. So he may get gonorrhœa. We should be sorry to believe that all the *inflammations* of mucous membranes belong to the same family.

Certainly, all this has something to do with Diagnosis. Who has not heard of the awkward embarrassments that have occurred here in our midst, to those who, unmindful of this present epidemic *genius*, could not see influenza through the disguise of typhoid fever, pneumonia, gastric or cerebral disease.

From the chapter on Rheumatism and Gout we extract the following paragraphs, which are fair samples of our author's style, and the intention of his work; p. 71:

"With reference to diagnosis, we have to discriminate diseases accompanied by pain in situations where chronic rheumatism is usually met with, and diseases of the joints, which are not rheumatic. The painful affections are chiefly neuralgic or sympathetic in the shoulder and upper part of the back; those connected with disorder of the liver and dyspepsia, across the loins; those produced by affection of the kidney, at the lower part of the back; in females, those associated with vaginal discharges and uterine disease. While in the hip and thigh it is often very difficult to make out whether the pain is of the ordinary rheumatic character, or is dependent on sciatica, which itself may be only a manifestation of rheumatism. In all of these cases we derive great aid from the consideration, that in rheumatic affections the pain is increased by movement: each, however, presents peculiar characters, which serve to confirm our diagnosis. In disorders of digestion, the prominence of the symptoms bearing more directly upon the function itself; in nephralgia and nephritis, the pain described as shooting down to the groin, thigh, or testicle; in uterine affections, the seat of pain corresponding to the sacrum, when movement cannot be its exciting cause; and in sciatica, the pain following the course of the nerve down the back and inside of the thigh, serves to discrimi-

nate it from one spoken of as extending from the hip to the ankle, simply because all the joints of the limb happen to be simultaneously affected."

"The diseases of the joints do not fall under our notice in medical diagnosis, but yet it is very necessary we should be able to satisfy our own minds whether, in any given case, there be not some more definite disease going on than that which, for want of more accurate knowledge, we call rheumatic. We have already noticed the degeneration of the synovial membrane, and we have still to mention ulceration of cartilages, scrofulous disease of the bones, and caries of the spine. In regard to all, we observe, that their course is very protracted, their commencement insidious, and that they are chiefly characterized by absence of pain in the early stage; pain, when it does come on, is shooting, transitory, and frequently attended by starting of the limbs. It is only in the advanced stages that it presents any permanence of character. We may further observe, not only that there is absence of pain in the quiescent state, but that cautious movement does not bring it on; while the slightest jar, causing concussion of one bone against another, is seen to cause pain, and that sometimes of a very severe kind. The patient who cannot bear his own weight on the ground, while perfectly motionless, is capable of much movement in bed without suffering, when the pressure is removed from the affected joint. A very frequent source of obscurity in the early history of these cases, is the circumstance that pain is not referred to the joint itself, but to a more distant one, in which it is only sympathetic. This point, when ascertained, serves as an additional ground for diagnosis. The age and aspect of the patient are suggestive with reference to the nature of the affection. Thus we look for disease of the bone in the young and delicate; ulceration of cartilages in the early part of adult life; and chronic rheumatism after its middle period, in persons who are fat and flabby, or cachectic and anæmic. In caries of the spine, the early progress of the case is scarcely marked by any feature which can distinguish it, till the prominence of one spinous process at the seat of pain indicates the irreparable mischief which has already taken place. In connection with this, we must not forget that lumbar and psoas abscess, or deep-seated pelvic inflammation and suppuration, when advancing slowly, are apt to simulate rheumatic affections of the loins and hips."

In chronic rheumatism we chiefly meet with two important complications, which seem each to have more or less to do with its development when present—constitutional syphilis and granular degeneration of the kidney. It is also intimately connected with mal-nutrition, whether tending to accumulation of fat or to general cachexia.

In the chapters on diseases of adventitious origin, we find syphilis and poisoning well considered—subjects which frequently offer embarrassments to the general practitioner, who is off his guard and unsuspecting—*causa latet, res ipsa notissima.*

The diagnosis of dropsies and hæmorrhages, chap. viii, does not present difficulties to those who are persevering in physical examinations, palpation, percussion, and minute examination of excreta.

Much of the art of diagnosis is better within the reach of any one of moderate ability and fair acquirements, who is patient and inquisitive—insists upon stripping his patient, and plunges, Curtius-like, into all the chasms—than of his superior in attainments, but of more elegant indolence and less brusquerie. It is the sharp rather than polite one who finds in the rectum cause for head symptoms, and, between bladder and rectum, cause for thoracic pain—who proceeds to amend a uterine hyperæmia before thrusting a probang into the pharynx. It is he, who is quite sure to examine suspicious coagula with his own eyes and forceps; and who listens for bruits below the umbilicus, before treating for amenorrhœa, or tapping for dropsy. He knows for a certainty whether beans or other lentils come out legitimately from the feminine urethra, and finds out whether "H. O., a female, aged about 16 years," vomits urine or not. Vid. Laycock's case, Ed. Med. & S. J., vol. xlix.

The chronic blood ailments, and depraved constitutional states, for some reason, are treated in separate chapters. In the first, chap. viii, are purpura, scurvy, pyæmia, chlorosis, &c. Two of these, scorbutus and purulent contamination, we are sure, frequently escape detection. Our dispensaries are crowded with the wretched victims, for whom lemon juice is the catholicon. We do not acknowledge that we have been converted to the scurvy doctrine, any more than to the psoric; but we confess that, if we had charge of a dispensary, Mediterranean fruit would be an item of expense to the establishment.

Pyohæmia deservedly attracts so much attention in the books and academies, and yet in practice eludes so often sharp investigations, that we refer with pleasure to the brief paragraphs in Dr. Barclay's work, pp. 105–107, which present the current doctrines as to the signs and nature of this insidious malady.

From the chapter which treats of scrofula, morbid growths, &c., which is quite as complete as the limits of the book would permit the author to make it, we find but little to transfer to our pages. The nature and locality of tumors are particularly well described, and we would only insist, rather more than Dr. B. finds it necessary to do, upon the diligent use of the pleximeter, Cammann's stethoscope, the exploring needle, Wyman's trocar, and upon digital examinations of the rectum and vagina, for the purpose of distinguishing pleuritic effusions, thoracic tumors, aneurisms, hepatic enlargements, muscular swelling, cœcal and vesical accumulations, uterine and ovarian en-

largements, &c. Some abdominal tumors, especially those formed by the gall bladder, distended, like that described in Cruveilhier's larger work, [Anatomie Path. xxix livr. pl. 4, fig. 1, 2, 3, and xxxvi livr. pl. 3 & 4,] and cysts formed by the kidneys and ureters, can only be made out exactly by the use of the pleximeter. The diagnosis of pancreatic tumors, like that described by Cruveilhier, [in the "Essai Sur l'Anatomie Path.," t. 1, p. 286, Paris, 1816,] we have reason to believe, will be much facilitated by the knowledge of auscultatory signs discovered by Dr. Cammann. Concerning tumors of this organ, Dr. Barclay says, p. 117:

"Occasionally a firm, hard tumor may be felt to the left of the epigastrium, which cannot be traced into the hypochondrium, and which, though accompanied by mal-nutrition, has not been associated with symptoms distinctly traceable to disease of the stomach. Such tumors have been found, after death, to be owing to scirrhus of the pancreas. The diagnosis is very difficult, and the position of the stomach is often such as to render it impossible to feel the hardened mass during life."

In considering the diagnosis of quasi-nervous diseases, pp. 124-126, our author gives some useful practical advice for the detection of that vexing, but profitable disease, Hysteria, to which the inexperienced might turn with profit.

The headings of Chap. XI. are deserving of note. They are, *General Examination of Regions and Organs—Diseases often Compound Phenomena—All Organs ought to be Examined—Negative as well as Positive results stated, &c.*

The chapter contains, among other things, a short-hand enumeration of the signs and symptoms which point to the different organs. When speaking of the kidneys, our author makes allusion to pain in the testicle. The pathological relation of these two glands has twice been vividly impressed upon our attention, by facts of an unfortunate character. Several years ago we saw, in the practice of a teacher of surgery, an inflamed testicle assiduously treated, locally, as a primary disease, for many weeks; till finally on the post-mortem table the diagnosis was changed, somewhat tardily, to correspond with the distended ureter, pus in the pelvis of the kidney, and a disorganized cortical structure. This blunder might have been avoided by a timely examination of the urine.

In the course of this chapter, Dr. Barclay gives this good advice to students, on the subject of note-taking, p. 128:

"One should never enter in his notes such vague expressions as 'chest healthy,' but state explicitly the extent of his examination and

its results, which need not, however, occupy much more space. Thus, to take the case of the chest, he may state simply that there is 'no complaints of pain, palpitation, cough, or shortness of breathing.' And this would imply that the chest had not been examined by percussion or auscultation. He may go further, and record that 'nothing abnormal has been discovered by percussion or auscultation,' or he may limit himself to some particular portion, 'breathing natural under the clavicles, at the back of the chest,' &c.; in the one case, he is understood to have examined the whole; in the other, only a part. The chief use of all these suggestions is to establish habits of accuracy; but if he should ever wish to refer to these cases in after-years, if it should be his lot to publish reports of them for the information of others, then the value of definite statements will more clearly appear."

The chapter on Semeiology of the Brain is worthy of special notice. It is not long, but includes much worth telling. We have not space to extract that which pleases us best, but find room only for a paragraph which contains Dr. B.'s opinions concerning a subject now much discussed in Germany and France. After remarking that the delirium of pneumonia, acute rheumatism, and erysipelas might readily be mistaken, our author says, pp. 137-138:

"Metastasis, in the true sense of the word, must be exceedingly rare in erysipelas; it may be conceived, but it is not known, as part of clinical history. I mean the disappearance of the swelling and redness of the part coincidently with the incursion of head symptoms. On the other hand, considering the nature of the disease, that it is not associated with exudation of lymph, but of serum, it is quite possible that serous effusion may be due to extension of erysipelatous inflammation to the membranes of the brain; but it is quite as probable that the delirium is merely the evidence of altered condition of the blood, of the circulation, and of the nervous energy, as in the other forms already noticed; and this is the more likely, because it exists without, as well as with effusion. In diagnosis, we have only to remember that the delirium of erysipelas is not associated with inflammation of the structure of the brain, or with such inflammation of its membranes as leads to effusion of lymph or of pus, because this is an all important point in treatment.

"In rheumatism, again, something very like metastasis to the heart occurs, and there may be something like metastasis to the brain. The disease is essentially erratic. But in far the larger number of cases, we are sure that the delirium does not depend upon metastasis, because rheumatic inflammation of the joints, of the heart, and of the pleura, is accompanied by exudation of lymph, and rheumatic inflammation of the membranes of the brain should be similarly evidenced by the presence of lymph. Post-mortem examinations prove that this is very rare indeed, and we are therefore justified in assuming, that unless the evidence of inflammation within the cranium derived from

other sources be very decided, delirium, following upon an accompanying acute rheumatism, is to be classed along with that of fever, of pneumonia, of erysipelas, and to be taken merely as evidence of blood-poisoning; and this so much the more certainly if there be no retrocession of the affection of the joints, or if it has been preceded by inflammation of the heart.

"In one of the less acute forms of rheumatism the synovial membrane is distended with serum, not with lymph, and several cases are on record in which the sudden disappearance of the effusion in the joint has been followed by an equally sudden occurrence of effusion in the brain. In these cases, the symptoms were rather of coma or stupor, than delirium; they, perhaps, are the only real instances of metastasis met with in practice."

The space devoted to diseases of the nervous centres is ample and is well filled—pp. 130–187. We note only an excellent resumé of what is known concerning the diagnosis of that frequent and perplexing malady—Infantile Meningitis. We have been accustomed, in thinking of the differential diagnosis of this *immedicabile vitium*, to recur to the Essays of Charles Taylor, on Infantile Remittent, &c., in the Medical Times, July, 1851; to Simon's, Golding Birds, and Rilliet's Essays on Cerebral Symptoms dependent upon diseases of the Kidney in infants; and the Guide du Medecin, of the pains-taking Valleix; but we find nothing more satisfactory than is contained in the work now before us. Reynolds, in his excellent "Diagnosis of Diseases of Brain, Spinal Cord, &c.," laments that works on Diagnosis are generally of no use till the disease has been ascertained. This objection, we do not hesitate to say, cannot be applied to Dr. Barclay's work, although we discover frequent evidence that the author had been accustomed to write of disease from its anatomico-pathological side, and so, forgetting now and then the intention of this work, has fallen into the disquisition style of the systematic treatise on the practice of medicine.

By no class of diseases is the art of Diagnosis more frequently put to the test than by those which present symptoms of pulmonary affection. For these, the grand question is often Prognosis, for which Diagnosis is an essential preliminary. No one, however expert at auscultation and percussion, should feel that he has exhausted the subject of the physical examination of the chest till he has read Addison's cogent resumé of the common fallacies by which those who assume for stethoscopic revelations unerring certainty, are endangered. Dr. Barclay is so judicious in this respect that we need offer no caution to those who carefully read his copious and minute exposition of the signs derivable from the examination of the sputa, and the various auditory phenomena of the respiration and circulation.

The chapter devoted to the general subject, examination of the chest, occupies 43 pages; and the special diseases occupy 60 pages more; quite sufficient for the faithful treatment of the subject, in our author's solid style. We have only to specify a deficiency in the section, p. 283, belonging to Diseases of the Lung in Childhood. There is here much that is deserving of attention, but the distinction is not clearly stated between lobar pneumonia in children, which is almost never fatal, and the tubular pneumonia, forever associated with the name of *Fauvel*, a disease almost always fatal and not uniformly apparent to auscultation. The following paragraph deserves to be remembered:

"Chronic bronchitis, which so often simulates, or is so often simulated by phthisis in the adult, is often quite undistinguishable from tubercular disease in infancy. Here dissemination of tubercle is the rule; its aggregation in masses, except in the bronchial glands, the exception. Hæmoptysis in childhood is by no means a sign of phthisis. The stethoscope can scarcely afford any assistance in discriminating these affections. It must be added that, when judicious treatment is employed in cases which have all the aspect of tubercle, the children so completely recover from the attack of bronchial irritation accompanying it, and are so often lost sight of subsequently, that no person of any experience will venture to give a decided opinion, except in very clearly marked examples of each disease. It is to be remembered that when the bronchial affection has passed, the signs of remaining consolidation at the apex are never found in childhood; if any localization of tubercle prevail at this period of life, it is only in the glands at the root of the lungs."

This subject is exceedingly well presented in a memoir "*Sur le diagnostic des maladies de la poitrine chez les enfants*," *par* C. Taupin. The little work of Dr. Racle, Paris, 1854, composed almost under the eyes of M. Bouillaud, may also be referred to as a very complete and convenient treatise on this subject.

The following sentences are noteworthy, as containing opinions concerning the nature and locality of croup, and are interesting in relation to the presence of tonsillar exudation as a diagnostic sign, and also from their bearings upon the question of tracheotomy. In this part of the United States the prevalent opinion differs somewhat from the following views of Dr. B., who, however, expresses what we have been accustomed to regard as the true pathology of the varieties of genuine croup; (p. 36.)

"Acute inflammations of the entrance of the air-passages in childhood is an affection quite *sui generis*. It is not here our business to enter upon its pathology, but merely to point out that,

while in the adult the inflammation is usually limited to the larynx, or at least derives all its importance from the inflammation attaching itself to the opening of the glottis, in childhood the trachea is the chief seat of the inflammation; the larynx and the fauces are usually involved secondarily, and to a less degree. The chief exception to this is found in the diphtheritis, which often prevails epidemically on the Continent; it clearly commences in the upper part of the pharynx, and very often terminates in true croup. As the disease proceeds, membranous shreds of lymph may be coughed up or expelled by vomiting, or patches of lymph may be seen on the fauces; this renders the diagnosis of the disease quite certain; but in some cases no membrane at all is found, the trachea and bronchia are simply inflamed and bathed in purulent secretion."

The remaining parts of this work are taken up with the consideration of Diseases of the Heart, Diseases of the Abdominal Organs, Examination of the Urine, and Diseases of the Skin. There is much matter that we should be glad to transfer to our pages, but as it is rather our intention to call attention to the doctrines of Dr. Barclay's excellent manual than to reprint it entirely, we refer our readers to the work itself.

The perusal of the book has afforded us much satisfaction. We welcomed it at first on account of its title, for it promised, we believed, to be the right thing for clinical students, and it appears to have fulfilled its intentions. It would not be difficult to find some deficiencies, for now-a-days people say to all writers, *approfondissez les choses*. To fulfill this requisition would have demanded of Dr. Barclay a larger book than he has undertaken. Much of the work is quite elementary, but it is not the worse for that; the flower and fruit of one generation make the germinal elements for the next.

But now, having gone all over this manual, the design of which is to teach men how to diagnosticate, there remains this to be thought of—rules, good rules, the best that can be, are not hard to find, but when all has been said, to diagnosticate is really and fundamentally a matter of *organization*, however expert you may be with the technics. We learn how, by means of the process of comparison, and by exclusion, which Rostan has ably developed, and the process of "*subsumption*," to use Siebert's expression, by inquiring what the given specimen of disease cannot be, and then what it must be, extraordinary feats of grand and lofty diagnosis have been accomplished by clinical masters. To successive generations of students, the stories current at La Charité of Cruvéilhier's and Velpeau's diagnostic leaps are handed down *sine grano*. The detection of inflammation limited to the splenic vein, and of an undeveloped fœtus in a scrotal tumor, are matters, we

have been made to believe, requiring no uncommon degree of acumen, if, indeed, they are not to be detected across the ward by a *coup d'œil.* With us, it stands as an article of faith, that it has been organically constituted, so that a prompt diagnostician is a man of fineness of fibre, an impressible man; one who looks for what he wants, and sees all there is before him, and foresees all there is to be. He has the faculty of separating himself for the minute from his past—leaves his memory, his preconceptions, his prejudices, and is nothing for the nonce but eyes and ears and tactile nerves. Who can do this? Not many, as we have seen men. To some, it is as certainly impossible, by any course of training, as it is to others to feel a logical compulsion, or an æsthetic nicety. The naturalist, with the true gift, has something of an artist's temperament, and may now and then be indiscreetly guilty of *des eclats de verve.*

The bovine temperament carries weight, and, with the selfishness of large vitality, looks out for its hay while the sun shines; but with all its strength, it has never done much to help on the freighted car of science. It must be true, for the "autocrat of the breakfast table" says it is, and he is too intimate with the professor, and shows himself too keen a naturalist, to be guilty of an error of observation—"that the bulbous fellows who steam well when they are at work, are the men that draw big audiences, and give us marrowy books and pictures." And Carlyle says of Mirabeau, the *fond gaillard,* "there is the same burly, thick-necked strength of body as of soul." We have seen a clumsy Dr. Mirabeau hopelessly befogged in trying to carry over the billows the tender child of a broken-hearted mother. These men of strong principles and great steadiness of purpose, possessing what is called character rather than intellect, may make good leaders of the people, statesmen, or preachers; but they become balky doctors, and altogether unprogressive, though adepts at deportment, and impressive at consultations. We have known those whose esculapian wisdom, it is fair to suppose, was put into a *sachel,* and deposited in the pocket some time ago; and when it is taken out to air, there is an odor of crushed flowers about the place. Esculapius in lavender!

The coxcombry of learning may be tiresome, but is more tolerable and praiseworthy than the Jesuitical *Sans-souci* with which bigheaded ignorance shades its eyes from the glare of new facts and the haze of tentative theory. It is not easy to stoop over a fatty omentum, and a distended colon is as much in the way of deference and humility as a torticollis is in the way of courtesy. He who walks with a heavy tread cannot change his course—not he. It is a clear case.

℞. Hydrargyri Chloridi, gr. xv.
Pulv. Jalapæ, gr. xxx.
M. Omne nocte sumend.

* * * * "What's to do?

Brutus.—A piece of work that will make *sick* men whole.
Ligarius.—But are not some whole that we must make *sick*?"

It may be said that saints without bodies are not the men for great deeds, but it is, nevertheless, their mission to put ideas into the heads of buncombe saints, who distribute them with lung and muscle. We should confess to the "Autocrat," if we had the honor of meeting him at his breakfast table, that it was one of his own offspring that put us up to making the remark, that your sharp, knobby, compact, full-blooded fillies make better time on the course of life and death than your heavy, sleek, complacent, steady goers of hepatic dignity, and should ask him if he did not agree with us, that the *vir artis medendi peritissimus* might quite as well be symbolized by a pepper berry as by a pumpkin.

Now, we hold, that in every case is a novelty which this sort of stereotype doctor is not prepared for. His mind is made up. He knows all about it. He learned how to treat fever forty years ago, and has failed to see that nature never exactly repeats herself—that even the sun's orbit is never twice alike. But he takes no note of Precession of the Equinoxes. The man who boasts of his consistency is possibly a correct man in morals, but is not likely to hit it right in medicine, though he swear by Pallas Athene, Asclepios, and Hygeia. He leans too much on his past, and is not sufficiently alive to the fact just now before his eyes. A strong nature moulds the molten mass that genius has smelted out of hard rocks. The patient waiters on the shore, with composure and safety, pick up the pearls of truth that are brought to their feet by the great world's tides. As they are self-preservative and venture nothing, they get only treasure trove by their searchings. The best pearls lie deepest, and are seized and brought to light only by those who, adventurous, are tossed at sea, and who, in pursuit of hidden gems, lose sight of themselves, and are at times lost to view of their fellows. SOIXANTE-DIX.

Essay on the Secretory and the Excito-Secretory System of Nerves in their relations to Physiology and Pathology. By HENRY FRASER CAMPBELL, A.M., M.D., Professor of Special and Comparative Anatomy in the Medical College of Georgia, (Augusta,) and one of the Vice Presidents of the American Medical Association. Philadelphia: J. B. Lippincott & Co., pp. 154, octavo, 1857.

The volume before us is made up of four essays, which we propose to notice very briefly, in the order in which they occur in the volume. The *first* relates to the "*Classification of Febrile Diseases by their relation to the Nervous System.*" As the normal functions of the human organism are known to occur under the superintending influence of the nervous system, Dr. Campbell concludes it rational to infer that all *abnormal* phenomena of life, as related to functional condition, are more or less influenced in their manifestations by "*aberrated*" nervous action. With this inference before him, he concludes that the classification of febrile diseases, at least, should be based upon the physiology of the nervous system. We cannot do better than to let the author speak for himself. "As in the nervous system we recognize two grand departments, viz: 1st, the cerebro-spinal system, all the normal actions of which are subject to cessations and interruptions; and 2dly, the ganglionic system, all the normal actions of which are of a continuous and *un*interrupted character—so in the manifestations of febrile diseases do we distinctly recognize two grand distinguishing characteristics, respectively typifying the *normal* actions of these two systems of nerves: thus, a character of *paroxysm* obtains in certain cases, while a character of *continuousness* as plainly marks the others." Again: the normal action of the cerebro-spinal system of nerves pertains almost exclusively to sensation and to motion, while in the normal action of the ganglionic system the entire function relates to nutrition and the secretions. To show the nervous relations of febrile diseases, it is only necessary to mention that *paroxysmal fevers* are characterized by modified sensation, while in *continued fevers* altered secretion and nutrition are the marked and most prominent characteristics. Upon these facts Dr. Campbell bases his classification of febrile diseases, making two grand divisions, thus: 1st, *cerebro-spinal fevers, paroxysmal* in character, the secretions and nutrition only *secondarily* affected; 2dly, *ganglionic fevers, continued* in character, the secretions and nutrition *primarily* affected.

It might be objected that such classification would avail us but little, as it would necessitate subdivisions not very dissimilar from our present nomenclature; for pneumonia, dysentery, &c., would necessarily fall under the head of cerebro-spinal fevers, while small-pox, mea-

sles, scarlet fever, &c., would be classified with typhus, under the head of ganglionic fevers. We did not, however, in this paper, propose a criticism, as that would involve more time and require more space than is at our disposal; but, rather, to give our readers some idea of the nature and importance of the contents of the volume before us. We may, however, remark, somewhat in support of Dr. Campbell's classification, that it is admitted by most pathologists, that remittent fever has its proximate cause in the nervous centre, and also that typhoid fever has its primary seat, likewise, in some yet undefined portion of the nervous system; yet it is manifest that these two diseases are essentially dissimilar in nature, are generated by conditions totally unlike, and that they commence their attacks by making impressions having no real analogy. It is true, we may get similar functional derangements in both diseases; but this derangement is, in the one case, the result of increased action; in the other, of diminished vitality. An obstruction to functional manifestation should not be confounded with a diminution of motive power. In additional confirmation of the pathology upon which Dr. Campbell bases his classification, we may observe, that Dr. Selade, Dumas, and others, have alluded to the frequent passage of intermittent fever into epilepsy, and from this fact have argued an analogy between these two diseases. Dr. E. Brown-Séquard, in a recent work on epilepsy, mentions the following fact, as bearing upon this point: "That intermittent fever is an affection of the nervous system, is proved by a curious case of fracture of the spine, in which the parts paralyzed remained in a normal state, while the rest of the body had all the phenomena of a paroxysm of fever and ague."

The second paper is entitled "*An Inquiry into the Nature of Typhoidal Fevers.*" This paper occupies sixty pages of the volume before us, and its principal object is to prove that typhoid fever has its pathological seat in the ganglionic nervous centres. We would be glad to follow the author, step by step, from commencement to conclusion, but that would require much more space than is at our disposal. In the absence of this, we take pleasure in referring our readers to the paper itself, which will abundantly repay perusal and study. We cannot let this paper pass without making one extract, which we have faith to believe will be sufficient to induce the readers of the MONTHLY to read the paper entire.

"Our knowledge in regard to its (typhoid fever) observed *phenomena* and *facts* is clear, well defined, almost certain; to complete the science in regard to it, it has but remained that these cognate,

well-ascertained facts and phenomena be rationally and correctly *interpreted*, that its true pathology might be *deduced*. To this arduous, though not unpleasing task, not without many misgivings, we have earnestly and diligently devoted ourselves; more with the hope that our labors would prove suggestive to others, of the true mode of arriving at real pathology, than that we should be able to supply the want or remedy the deficiency. Starting with what we considered the rational assumption that the pathology of typhoidal fevers is in the ganglionic system of nerves, we have compared their characteristic traits and phenomena with, first, the *normal* action of this portion of the nervous system, then with the known and well-established results of *experimental irritation and action* of various portions of these nerves, and we have found that the analogy is sufficiently close to admit the legitimate inference that the symptoms and pathological lesions of typhoid and typhus fever are produced by an abnormal action in certain portions of this system of nerves. First, however, no typhoid or typhus phenomenon ever occurs, except in regions supplied by this system; secondly, because the peculiar phenomena of these diseases occur in a more marked degree in those parts most abundantly supplied from this source; and thirdly, because the nature of these symptoms is always found more purely and characteristically typhoid in those portions of the organism supplied exclusively by this kind of innervation. And further, on the other hand, we are forced to admit the truth of these impressions, because we have hitherto had no theory or legitimate and consonant combinations of theories, to our mind, as competent to the full and rational explanation of *all* the phenomena of the disease, as the one now offered." (p. 58.)

In drawing a distinction between typhus and typhoid fevers, Dr. Campbell says, "we would locate *typhoid fever* in the *visceral* portion of the ganglionic system, and *typhus* in the *vertebral* portion."

The *third* paper is entitled "*The Excito-Secretory System of Nerves: its relations to Physiology and Pathology.*" To this essay the prize of one hundred dollars was awarded at the tenth annual meeting of the American Medical Association, and is published in the last volume of the Transactions of the Association. This essay occupies about 50 pages of the volume, and gives evidence of much study and careful thought. We have carefully read this essay, and marked several passages for quotation and remark; but we now forego our purpose, for to do the author justice would require much space, and we are the more willing to forego this analysis, knowing that our readers, interested in this subject, will avail themselves of the opportunity to read the essay entire. The object of the paper is thus distinctly stated by the author: "Our earnest endeavor has been to bring together, from all sources, however diverse and unpromising, a

sufficient number of kindred facts, to establish more fully, perhaps, than has heretofore been done, the physiological and pathological relations of the excito-secretory system of nerves." To this task, uninviting and even forbidding in some of its aspects, he has devoted himself, in the full conviction that the field is a rich one. Such it has proved in his hands.

The fourth and concluding paper is entitled "*A Claim of Priority in the Discovery and Naming of the Excito-Secretory System of Nerves.*" This was prepared originally for the *London Lancet*, and was addressed to Dr. Marshall Hall. This paper, in which Dr. Campbell urges his claim to priority in discovering and naming the excito-secretory system of nerves, elicited a response from Dr. Marshall Hall, under date of April, 1857, in which he generously yielded to Dr. Campbell all that he claimed as his right.

In conclusion, we would say this volume will well repay an attentive perusal, and is certainly creditable to the author and to American medicine. O. C. G.

Proceedings of the American Pharmaceutical Association at the Sixth Annual Meeting, held in Philadelphia, September, 1857; *with the Constitution and List of the Members.*

This is the title of a thin octavo volume of 178 pages, which exhibits very gratifying indications of the advance that is being made by the pharmaceutists of our country. But a few years have elapsed since this association was formed, almost without attracting any notice from the public mind, and now we find their proceedings containing papers indicative of careful research and zealous study. The reports, generally, indicate a true scientific spirit, which we hope will eventuate in driving from the shelves of every educated pharmaceutist all nostrums and quack medicines. The report on weights and measures suggests that there be but one system of each, depending on a decimal character for its value. The unit for the system of weights proposed is the present *avoirdupois* pound. Ascending by tens, from this unit, are stones and hundred weights; descending, ounces, drachms, scruples, and grains. The unit for the system of measures is the gallon, which must contain exactly 10 pounds of distilled water; ascending from this, we have ankers and tons; descending, pints, ounces, drachms, and scruples. "*The Report on Sale of Poisons*" contains some valuable suggestions as to the proper precautions to be exercised so as to pre-

vent accident. "The Report on the Progress of Pharmacy" contains the following, which clearly shows that there are some non-progressives in the business even at this day. A member of the committee writes, "I am acquainted with one doing a considerable business, who had only to aid him an old London Dispensatory of 1796, and within the last six years he asked me seriously what dispensatory I would advise him to purchase, as he found that so many new articles were introduced which he could not find in the old one, it was necessary to get another." "The Report on Unofficinal Formulæ," though tolerably full, is not as much so as one could desire, when the immense number of these formulæ is taken into consideration.

In addition to these reports, there are some reports and essays on special scientific subjects, viz.: New England Isinglass, the manufacture of Iodine, Henbane of American growth, American Lard, Elaterium, Ergot, Spigelia, methods of rendering medicinal preparations pleasing to the eye, &c., the use of indigenous plants, and an article on ethical analysis, (sic!) which is occupied with certain queries as to the duties of pharmaceutists.

On the whole, we think this little book is very creditable to the Association whose imprimatur it bears, and will be profitable, on account of its information, to all its readers. S.

Report of the Trial of Miss Madeline Smith for the alleged Poisoning of Pierre Emile L'Angelier, revised and corrected by JOHN MORISON, Esq., Advocate. Edinburgh: W. P. Nimmo, 1857, pp. 184.

The case of Miss Smith claimed public attention immediately after that of Palmer. In the latter, the poison was not detected, but the chain of circumstantial evidence was so overwhelming that a verdict of guilty was rendered by the jury; in the former, the poison, arsenic, was detected in the small intestine and contents, large intestine, liver, brain, heart, and lungs, but the evidence did not justify the jury in fixing the crime of its administration on the prisoner. The report of the case is interesting to the toxicologist, not on account of any difficulties that attended the chemical examination of the organic substances, but because of the readiness with which the regular reactions of arsenic were obtained by Dr. Penny, of the Andersonian University, Glasgow. On the copper foil used in Reinsch's process, an abundant steel-like coating was obtained, which gave off the arsenical odor on being heated in a test tube, and was converted into arsenious acid. Marsh's

process, with a distilled portion of the fluid, gave arseniureted hydrogen gas, with its bluish-white flame, and the characteristic properties of this gas. Sulphureted hydrogen precipitated the ter-sulphide, soluble in ammonia, and unchanged in chlorhydric acid. Many years of troublesome investigation were required before chemistry was able to present its present simple series of tests for arsenic, but their very simplicity now bids us take courage as to our future ability to determine poisons at present considered as beyond the reach of chemical investigation. S.

PROCEEDINGS OF SOCIETIES.

Academy of Medicine.—Obstetric Section. Dr. BARKER, Chairman; Dr. UNDERHILL, Secretary.

Jan 18th. After the election of officers for the ensuing year, and passing a vote of thanks to the retiring officers, the relation of cases being now in order, Dr. Underhill was called upon, who related the following case of

Menorrhagia, occurring in a married woman some 45 or 46 years of age, and the mother of five or six children. Her health had previously been good, although she was of rather a delicate and nervous constitution. She had menstruated regularly up to the time of the present attack. For a month previous to his being called to see her, there had been a constant discharge of blood from the uterus. He found her in bed, with a pale and sallow countenance, throbbings in the head, dimness of sight, tongue coated, pulse very weak, 90 per minute. Upon a vaginal examination, he could discover nothing abnormal in the condition of the uterus. The os was slightly open. The treatment consisted of elixir vitriol internally and cold applications to the vulva. Afterwards, ice within the vagina, ergot, acetate of lead, and gallic acid. When the hæmorrhage ceased, a troublesome nausea and vomiting came on. She was at length able to move about the house, and she gradually gained strength. In two weeks the hæmorrhage again came on profusely, and was again arrested by the use of some of the remedies before mentioned, special reliance being placed on the gallic acid and acetate of lead. On the cessation of hæmorrhage the vomiting again recurred as before, on account of which a blister was applied to the epigastrium.

Dr. White ascribed the hæmorrhage to the turn of life. Dr. Bar-

ker said he had found the menorrhagia of the critical period exceedingly difficult to manage when it occurred with no rational or physical signs of uterine disease, and he, in common with all present, had met with many such cases. In the present state of uterine pathology we are accustomed to search carefully for structural changes, when such an important functional disturbance occurs; but for years he had constantly more or less cases of troublesome menorrhagia at the critical period in which no uterine or ovarian disease could be detected, and apart from this one symptom and its consequences there was no disease of any other part of the system which could account for the tendency to uterine hæmorrhage. He had tried every variety of astringent and styptic, both locally and internally, but no plan of treatment had proved satisfactory. The ergot, so highly lauded by Churchill and others; the gallid acid, cinnamon, as recommended by Dr. Tanner; matico, turpentine, and, in short, all this class of agents had failed so frequently in his hands, that he now placed no faith in them in this form of hæmorrhage. As to the acetate of lead, he could not understand how it ever obtained so much reputation as an agent for controlling any form of *uterine* hæmorrhage. Dr. B. used it for many years, in a great number of cases, and in the fullest doses, without obtaining such results as would lead him to place much reliance upon it. It is now nearly two years since he had adopted a new method of treating the menorrhagia which occurs at the critical period, and the results were so satisfactory both in his own practice and in that of other gentlemen who had tried it, that he felt justified in mentioning it to the section. Menstruation is ordinarily coincident with ovulation, and the present doctrine is, that it is a result of an abortive attempt on the part of the uterus to form a decidual membrane. In other words, it is a kind of ovular lochia, accompanied by a similar change in the lining surface of the uterus, (differing only in degree,) as that which follows parturition. Now, starting from these physiological premises, by a process of reasoning which will readily suggest itself to every one, the theory was deduced, that this hæmorrhage resulted from a lesion of the internal surface of the uterus. Therapeutical results seem strongly to confirm this view. Of course, it cannot be demonstrated to be true, except by careful necroscopic examinations, an opportunity for which seldom occurs. The treatment consists in applying remedial agents directly to the internal surface of the uterus. At first, Dr. B. tried injections into the cavity of the uterus, but these almost invariably caused severe pain. Any fluid thrown into the cavity of the uterus will generally excite uterine colics. It

is a curious but a well-established clinical fact, that the uterus tolerates within the cavity a solid substance much better than a fluid. He (Dr. B.) often found patients who suffered very little pain from the introduction of the sound and any necessary manipulations with it, who complained of severe sufferings, lasting even for hours, when a few drops of any fluid was thrown into the cavity of the uterus. It should be mentioned, however, that where the cavity of the uterus is greatly enlarged by sub-mucous fibrous tumors for example, the tolerance of fluids is very much greater. In two cases, with such tumors, Dr. B. has thrown a half ounce of the Tinc. Benzoin Co. into the uterine cavity with the effect of at once arresting the hæmorrhage without producing pain. The only sensation excited was a moderate degree of warmth. But in those climacteric hæmorrhages, unassociated with any abnormal growth, Dr. B. now uses the following:

No. 1.

℞.—Argent. nit. crys. ℥ij.
Ext. Belladon. ʒj.
Ung. Cetacei, ʒi.
M.

No. 2.

℞—Zinci. Sulph. ℥ij.
Glycerin, ʒij.
M.

These are carried into the uterine cavity by means of a kind of suppository tube. From one to two inches of the tube is filled with the ointment, the tube is carried into the cavity of the uterus in the same way as the sound is introduced, the piston is then pushed home, which leaves the ointment in the cavity. By this treatment he has found these cases perfectly manageable. Not to detain the section by a detail of cases, he would only mention one which he saw with Dr. Kissam. The patient had for a long time been suffering from uterine hæmorrhage, and was extremely emaciated. One application of the zinc and glycerin was followed by complete arrest of the hæmorrhage, and more than four months have now passed without any recurrence. Dr. B. was not able to say which formula was the more effective, but the principle which guided him was this: when the uterus was somewhat enlarged, irritable, and painful on pressure upwards through the posterior vaginal cul de sac, he used the nitrate of silver and belladonna ointment. When these symptoms were absent, he selected the zinc and glycerin.

Dr. Wooster has succeeded in controlling this form of menorrhagia

by injecting nitrate of silver into the cavity of the uterus. He had not found these injections produce so much pain as Dr. Barker's remarks would imply.

Dr. Taylor remarked that sulphate of zinc will sometimes act as a powerful escharotic, and he should therefore be very apprehensive that its use in the manner described might produce ulceration.

EDITORIAL AND MISCELLANEOUS.

The Academy of Medicine held its regular meeting for March on Wednesday evening, March 3d. After the reading of the minutes of the preceding meeting, Dr. John McNulty asked permission to make a personal explanation. It appeared that Dr. M. was not present at the February meeting, having gone to Albany as a delegate to the State Medical Society, and that during his absence advantage was taken to attack him, as he believed, unjustly and unfairly. A good deal of feeling was manifested by those to whom the doctor attributed these unworthy actions and motives, and as he insisted upon calling things by their names, instead of using doubtful circumlocutions, the discussion was animated. He was repeatedly called to order by members, but having found that he was in order, was allowed to go on. When silenced by the president, he appealed to the Academy, and the Academy sustained him; and he continued till, as was understood, at the suggestion of one of his friends, he postponed the farther discussion of the subject.

The reports of sections being next in order, the section on surgery and the section on obstetrics reported. By the latter a complimentary resolution was proposed on the paper of Dr. Thomas upon prolapsus of the funis, and it was adopted by the Academy. A long written report was presented by Dr. Janes, secretary of the section on materia medica. Some time since a quantity of fluid extracts was presented to the Academy by the manufacturers of them, and one or two partial reports from the section have been referred back for further consideration. This one was a high commendation of fluid extracts as a mode of obtaining the valuable ingredients of medicinal plants, and seemed to surprise the Academy, who had before received a paper from a member of the same section, which demonstrated the uncertainty of all such preparations. As this was presented as being the unanimous opinion of the section, Dr. Rotton, who had read the paper

just referred to, said that, as a member of the section, he entirely dissented from the report, and asked leave to present a minority report, which was granted to him, together with permission to use the majority report in preparation of his. It was proposed that the report of the section be allowed to be printed, but it had so much the look of an advertising dodge and puff for the manufacturers, that permission was not granted.

The secretary read an invitation from the executive committee of the quarantine convention to the Academy to send delegates to the next meeting, to be held at Baltimore in May next. The communication was received, and it was voted not to send delegates.

The secretary then read a communication from the County Medical Society of Philadelphia, which had, by vote of the Academy, been ordered for reading this evening. The paper was a complaint against Dr. D. M. Reese, of this city, because he recommended Dr. McClintock, of Philadelphia, to the Board of Health of that city as a man qualified to take charge of Blockley Hospital. Dr. McC., it appears, some few years ago, furnished to certain parties, for a pecuniary consideration, the prescriptions for various preparations, which were afterwards advertised for general sale as the McClintock medicines. This was a great departure from the rules of medical propriety, and Dr. McC. is alleged to have thus forfeited his position as an honorable member of the profession. For this conduct he was expelled from the American Medical Association; and the County Society of Philadelphia complain that Dr. Reese, who is now vice-president of the same association, should have given him a recommendation. The complaint was lugubrious in the extreme.

The secretary moved that it be referred to the committee on ethics. Dr. Reese then took the floor, and objected to the reference to that committee. By their previous action, he contended, they had put themselves in a position entirely disqualifying them from farther consideration of the subject. They reported that the paper contained "grave charges," and instead of confronting the accused with the accuser, or giving the accused a written copy of the charges, or endeavoring "to reclaim the offender"—all of which things the constitution makes it their duty to do—they had recommended the paper to be read openly and publicly, without first inquiring whether or not the charges were sustained by evidence, and thus had become Dr. Reese's accusers. As to his giving the recommendation, he admitted that he did so, and under these circumstances. Dr. McClintock and himself were, in their boyhood, friends and playmates; their parents' families

being intimately bound together, and themselves always attached to each other. McC. studied medicine at the Jefferson School in Philadelphia; was a pupil of Godman, and became his successor as an independent lecturer on anatomy, having frequently a class of three hundred pupils. Not content with this, he aspired to be professor in one of the schools in Philadelphia; but not succeeding, obtained from the Legislature a charter for another school, which he organized, and which entered upon its career. From this rivalry with the older schools comes all the hostility to McC., and this is the real cause of the complaint of the County Society. In this institution, he, as its president, became responsible for the salaries of the professors, and by these expenses and pecuniary responsibilities was ruined. Thus involved, he found himself very seriously ill, with congestion of the brain, and believed himself not likely to recover, or to live more than a year or two. His family consisted of a wife and nine children, seven of whom were girls, and one of the boys was an invalid, incapacitated for business. Thus situated, with death before him, and nothing but poverty to leave to his children, he was solicited to furnish to a company his prescriptions, and the use of his name, on condition that he should receive $5000 down, and $5000 a year subsequently, and his family to have six per cent. of the profits after his death. Were not these circumstances sufficient to tempt a man? Dr. Reese at once told him he had done wrong, and their friendly intercourse was entirely interrupted. But Dr. McC.'s health was restored, the speculation failed, and he sought to return to the practice of his profession. He was first elected to the Board of Health of Philadelphia, and retired from it without any reproach, after a year's service. In the spring of 1857, Dr. R. saw him at Philadelphia, and learned that he had abandoned quackery. Some weeks after his return home, he took out of the post office a letter, asking for a certificate that Dr. McC. was qualified to take charge of Blockley Hospital, it being added that the election would be the next day, and an immediate answer was necessary. Dr. R. immediately went into a neighboring stationer's, and hastily wrote that Dr. McC.'s professional attainments were such as to fit him for the place. He affixed no titles to his name, (which was one of the complaints of the Philadelphia Society,) but simply answered the question put to him, honestly and fearlessly. He did not pause to inquire what was politic, but what was his duty, and then did it. And he would add, that, under precisely similar circumstances, he would do just the same thing again, "now, henceforth and forever." The doctor then proceeded to comment on the respectability of the

source from which the communication came, a point insisted upon by the committee on ethics, saying that he had in his pocket a copy of the indictment against one of the signers of the paper of charges, on account of various crimes against property. In conclusion, he said if he had any friend there, he wished he would move a reference of the matter to a special committee. (During Dr. Reese's speech he was frequently called to order by various members; was interrupted by the president, who seemed to forget that he was a presiding officer, in his anxiety to defend his position as a member of the committee on ethics; and occasionally was greeted with cordial and hearty applause.)

Dr. Warren then rose to defend the committee. Dr. Barker moved to refer the matter to a special committee. Dr. Gardner moved to lay it on the table. Dr. Anderson made explanations for the ethical committee. Dr. H. Green thought there was no constitutional ground for action, and that the matter should be dismissed. And so the discussion went on, in the most irregular and exciting manner.

Finally, Dr. Detmold presented two resolutions, as substitutes for all other action. They were as follows:

Resolved, That this Academy learns with regret that one of its Fellows has recommended to a position of high respectability an individual who has forfeited his rank in the profession.

Resolved, That a copy of the resolution be sent to the Philadelphia County Medical Society.

The discussion then turned upon these resolutions. Dr. Beadle moved that the word *reprimand* should be introduced. Some one else moved that Dr. Reese's name be inserted; and then the previous question was demanded. After a protracted discussion, the ayes and noes were taken on the adoption of the first resolution, and it was carried by a vote of 34 to 16—the whole number of Fellows of the Academy being over one hundred. The Academy then adjourned.

—Monday, the first of March last, was the occasion of the first presentation of the two prizes offered by James R. Wood, M. D., for the best anatomical or surgical preparation, to be presented to the Bellevue Hospital, from the three schools of medicine in this city. Dr. Valentine Mott, who was assigned the duty of making the awards, stated that on the Saturday previous a commission had been appointed, consisting of the professors of anatomy and surgery in the three medical schools in this city, to examine the different specimens of surgical anatomy which had been prepared under the offer of Dr. Wood. They had performed their duty, which they had found most agreeable, and they had met now to render their decision. He spoke in high

terms of Dr. Wood as a medical man and brother surgeon, and the noble object which had incited him to make the offer of these prizes. He alluded to the value of anatomical surgery, and the great advances that had been made in that art since he first began to teach it in Columbia College in 1809. Taking a retrospective view of this department of science, he referred to the eminent labors of Cooper, Burns, Velpeau, and many others; and then, in terms of warm eulogy, directed attention to the very beautiful preparations to which the commission had awarded the prizes.

The first prize, of fifty dollars, was awarded to George F. Shrady, since a graduate of the College of Physicians and Surgeons, Twenty-third Street. Dr. Mott stated that he had seen a great many dissections, and had made many himself, but he was willing to say that, not in his own museum, nor any where that he knew of, was there any specimen where so many branches of the trifacial and seventh pair had been followed up to their extreme ramifications as were exhibited in this specimen, which, he said, did infinite honor to the gentleman, and credit to the age in which we live; and furthermore, that he should have been proud to have been the maker of that specimen himself. As an old man, who for fifty years had been interested in this subject, he expressed his conviction of a high and glorious future before its maker.

Dr. Shrady's preparation consists of the upper half of the body of a baboon, with the nerves and vessels of that part. The calvarium is sawn off, exposing the twelve pairs of nerves entering the foramina, at the base of the skull. On one side of the face is a beautiful dissection of the seventh pair, with its inosculations with the fifth and anterior cervical nerves. The other side of the face exhibits also the seventh pair, through the intricate meshes of which is seen the fifth pair complete, not even omitting Meckel's ganglion. The head is thrown back, to show the nerves in the anterior cervical region, with its vessels. The anterior cervical nerves are very elaborately dissected, constituting one mass of fibres on each side of the neck. The posterior cervical, also, shows to great advantage. In the chest, we notice the sympathetic, phrenic, pneumogastric, and intercostal nerves. Also the position of great vessels coming from the heart. The brachial plexus is followed down to the bend of the elbow. Dr. Shrady is now at the New York Hospital, as senior walker to the surgical division.

The second prize, of twenty-five dollars, was awarded to George E. Post, of the University Medical College. It consisted of a prepara-

tion of the sympathetic of the head and neck, with its connections with the nerves of that part of the body. The specimen is small, but shows a great amount of labor spent upon it, by the minuteness with which the dissection is carried out.

These prizes were accompanied by beautiful testimonials, signed by all the members of the commission—Drs. V. Mott, A. H. Stevens, W. Parker, R. Watts, A. C. Post, W. H. Van Buren, E. R. Peaslee, and T. C. Childs. The awards were received with loud applause.

Dr. J. W. Francis then rose, at the request of Dr. Mott, and addressed the audience at some length. He traced the history of preservative anatomy, as he called it, from the time of the celebrated Ruysch, of Amsterdam, who began to practice it in 1630, down to our own days. Ruysch, he said, died at the age of 93, a marvel to all who passed through Holland, after having labored in his art for more than 60 years, and for many hours a day. The czar, Peter the Great, repeatedly left Russia to come to Holland to admire his specimens, and take his breakfast with him; and, as the story goes, they generally had a blow-out together before they parted. [Laughter.] The czar offered Ruysch £25,000 for his museum. The offer was accepted, and Holland had to part with that magnificent possession. But Ruysch, though in his 60th year, set to work again. Having an active son and a wonderfully clever anatomical daughter, who dissected every thing, no matter what, he recommenced his labors, and soon accumulated a new museum. It was a singular fact that, in all their physical investigations, these old professors always had their children at their elbows. The very illustrations for Blumenbach's anatomical dissertations were made by his daughter, a tall, raw-boned girl. This mode of instruction was adopted about 1677–1700–1710 Paris closely followed Holland, and Edinburgh adopted it soon after Paris. About thirty years after Monroe began in Edinburgh, Hunter made anatomical preparations in Rhode Island. Philadelphia, though she had now a good collection, was the last of all the American cities to exhibit any enterprise in this regard. In coming to New York, we found that Dr. Post had brought out to this country a large collection particularly illustrating the absorbent system, because he had studied under Cruikshanks, who wrote on absorbents. He also brought out many specimens of vascular preparations, because he had also been a student of John Hunter. But why did he not bring out some of the remarkable specimens of John Sheldon, another of the teachers? The reason was that Sheldon kept the peculiar secret of his art to himself, just as Ruysch did. There was a plan adopted by these men of giv-

ing a color and efflorescence to the skin of the subject, which the moderns do not seem to understand. John Sheldon had fallen in love with and married a very beautiful lady, who died. He was determined not to part with her, even though death had occurred, and he had her made into a preparation, and for more than thirty years this beautiful anatomical specimen, fresh as in life, was preserved by him. Sheldon died in the year 1800, Professor of the Anatomy of Painting to the Royal Academy of Arts, having succeeded the Hunters. In a book of travels, written by Dr. Mott thirty years ago, he (Dr. F.) remembered to have seen an account of a famous Italian anatomist, Segatto, who possessed the secret of making human flesh as hard as stone, so that he was able to make a mosaic table out of different pieces! Dr. Francis, after pursuing this subject somewhat further, concluded by expressing the hope that fortune might smile on all the young men who surrounded him; and he trusted that they would be as familiar with the subject of their studies as was Dr. Post, who, when a preparation was forced on his attention in a Parisian hospital, as being one extremely well done, remarked, "Yes, it is extremely well done, but I am sorry to see the artery on the wrong side!"

Prof. Smith, of the College of Physicians and Surgeons, (23d Street,) made a few appropriate remarks, after which Dr. G. T. Elliot followed, and stated that he contemplated offering prizes for anatomical preparations in connection with obstetrics.

Dr. James R. Wood concluded the exercises, by remarking that the occasion was one of peculiar satisfaction to him. He was glad that he was seconded by Dr. Elliot, in connection with his offer for the prizes, and hoped in time to have a museum for Bellevue Hospital equal to any in the world. In referring to Dr. Shrady's specimen, he stated that it was nine months in preparing, but during that time the author had suffered severely from typhus fever, brought on by excessive application to the work; but it was a glorious effort, and would remain in the halls of the museum an honor and credit to the talented projector.

Dr. Mott called attention to the excellence of the other specimens presented, and he eulogized particularly the one by Mr. Bromley, of the New York Medical College, (13th Street,) and another by Mr. George K. Smith, of Broome County, N. Y., of the University of New York.

—The discussion upon puerperal fever, in the Academy of Medicine in this city, has attracted the attention of the more dignified, learned, and distinguished Academy of Paris. For this thanks ought to be given to the MONTHLY, (we are not very sanguine of receiving them,) which has published full reports of the discussion, and sent them to Europe. The result of the French discussion, which is commenced in our last exchanges, will be given by us as soon as it is received, and that, we have no doubt, will be long before our own society drags its slow length along to completing its undertaking. Meantime, as our friends in Paris are at a loss to know precisely what plant is spoken of by us under the name of *Veratrum Viride*, we copy its botanical description. In copying Dr. Vedder's case of puerperal fever, from the January number of the MONTHLY, the editor says: "It is undoubtedly the plant which we here designate by the name of green hellebore, (*helleborus viridis*,) of the genus hellebore, family ranunculaceæ." The learned editor is here in an error. The veratrum viride is indigenous to this country, and found from Canada to Carolina. It is of the same class (melanthaceæ) with colchicum, and may perhaps be more easily identified by European botanists by this technical description:

Order, Melanthaceæ—genus, Veratrum.

Flowers monœcious-polygamous; perianth deeply 6-parted, spreading; stamens 6, inserted on the receptacle; ovaries 3, united at base, often abortive; styles short; capsule 3, connate, many-seeded.

Lat. *vere, atrum*, truly black; alluding to the color of the root. Leaves alternate, plicate. Panicle terminal. Segments of the perianth sessile and without glands.

V. VIRIDE.

Leaves broad-oval, acuminate; *panicle* compound, racemose; *bracts* oblong-lanceolate, bracteoles longer than the downy pedicels. A large-leaved, coarse-looking plant, of our meadows and swamps. Root large, fleshy, with numerous long fibres. Stem 2; 4 feet high, striate and pubescent. Leaves strongly nerved and plaited, the lowest near a foot long and half as wide, sheathing at the base. Flowers numerous, green, in many axillary (or bracted) racemes, which together form a very large, pyramidal, terminal panicle. It will be seen that this is not the V. parviflorum of Michaux.

—We should, with pleasure, answer all the inquiries put to us concerning *Sewing Machines*, if it were in our power. Their name is getting to be legion, and each person needs to select the one that suits. This we can, however, say, that for the last year one of Messrs. Grover & Baker's machines has been an inmate of the family of one of the editors. It is quiet, except when at work, and then makes no

more noise than is necessary; does its work very handsomely and thoroughly; is not apt to get out of order; and, if it was impossible to replace it, would not now be spared on any condition. At present, we are making some investigations concerning other machines, and this much is already apparent, viz., that one must *learn* to use them, and not expect to be skillful at once, without having the necessary tuition. A person might just as well expect to run a locomotive when he had no knowledge of machinery.

—*Utile cum dulce.*—The useful with the agreeable, ought to be a motto conscientiously remembered by druggists and physicians in the compounding of prescriptions. We say it *ought* to be so, but are, at the same time, very certain that it is not. This does not hold good, however, of the preparation of Calisaya Bark, advertised by Messrs. Hegeman & Co. It is about two years since we first used it, and having ordered it for a large number of patients, feel authorized to speak of its excellence. It is a peculiarly agreeable medicine, pleasing the most fastidious, and yet possesses striking virtues as a tonic. For children, it is particularly adapted; while adults prefer it to the ordinary tinctures. To physicians, in this city, it has already come to be a necessity.

—*University of Maryland.*—Several changes have been made in the Faculty of this venerable school, at the end of the Lectures for 1857–8. Prof. R. H. Thomas resigned the Chair of Obstetrics, which he has held for some years, in consequence of ill health. This chair was filled by the appointment of Prof. G. W. Miltenberger, formerly Professor of Materia Medica and Therapeutics in the same school, and the vacancy occasioned by his transfer was filled by the election of Prof. Charles Frick, M.D.

— Mayor Tiemann has vetoed the resolution of the Common Council of this city, directing the Counsel of the Corporation to apply to the Legislature for the passage of an Act for authority to give a site for a building to the State Woman's Hospital. This site was the block of ground lying between Fourth Avenue, Lexington Avenue, Forty-ninth and Fiftieth Streets. He vetoed it with pain and regret, as his sympathy was with the institution. He would approve of any donation to this hospital, or any other began method by which the Common Council could afford relief to this charity.

—Dr. F. Eve has retired from the *Nashville Journal of Medicine*, and Dr. R. C. Foster, *4th*, and Dr. George S. Blackie, have joined Prof. Bowling, the remaining editor, in conducting this excellent journal.

—The Eleventh Annual Meeting of the American Medical Association, will be held in the City of Washington, on Tuesday, May 4th. The secretaries of all societies, and other bodies entitled to representation in the association, are requested to forward to the secretary, Dr. Alexander J. Semmes, corrected lists of their delegates as soon as they may be appointed.

—A new Journal is announced to appear in Paris, to be edited by Dr. E. Brown-Séquard, assisted by Drs. Robin and Tholozan. It is to be quarterly, of about 200 pages, and will be principally devoted to original articles, analyses of papers, and recent researches in physiological science.

—*Phosphate of Lime.*—Saint-Lager, of Lyons, has been experimenting as to the best form of this article for internal administration. It may be given in the three forms—of calcined bones, *precipitated* phosphate, or pulverized bones that have not been calcined. But which of these three forms will most readily enter into the general circulation? He digested, at 36°c., equal quantities of each in solutions, of equal strength, of citric, tartaric, acetic and lactic acids. The organic acid only dissolves a small quantity of the calcined bones, not attacking to a great extent the contained phosphate; whilst bones not calcined, placed in the same conditions, readily yield up their phosphate of lime, as is easily recognized by adding a few drops of ammonia, which will ensure an abundant precipitate. The phosphate of lime, obtained by precipitation, when recently prepared, is very promptly dissolved in these acids; but when old it dissolves slowly, and not as quickly as pulverized bones. Its preparation is somewhat slow, on account of the necessary washing. Saint-Lager thinks, therefore, that rasped or pulverized bones are preferable as sources of phosphate of lime. A. Chevallier considers the *hydrated phosphate of lime* as the only form which should be employed in medicine. He prepares it by dissolving calcined bones in chlorhydric acid, precipitating by potassa, receiving the precipitate on a filter, and washing with a large quantity of water. This should be preserved under a slight stratum of pure water.—*Jour. de Chim. Med.*

— This medical item is from "Brazil and the Brazilians, portrayed in historical and descriptive sketches." Has the disease ever been described by professional writers?

"At Lameisa I became aware of a new disease, which, like the *girtre*, seems to be confined to certain localities. I was sitting in the office of Dr. ———, conversing in regard to Brazil, when I observed a Portuguese, about sixty years of age, enter, and demand, with

great earnestness, if he thought that he could live. Soon after a middle-aged Brazilian came, and, seeming to cling to the words of the physician as tenaciously as to a divine oracle, made nearly the same interrogatory. Neither of these men appeared in ill health, and, if I had not heard them state that they had great difficulty in swallowing, I would have considered them in a perfect sanitary condition. Upon inquiry, I ascertained from the doctor that these men had a disease which is widely prevalent in some portions of interior Brazil, but he has never seen a notice of it in any medical work whatever. The Brazilians call it *mal de engasgo*. The first indication of its existence is a difficulty of swallowing. The patient can swallow dry substances better than fluids. Wine or milk can be drunk with more facility than water, still both are attended with difficulty; to take thin broth is an impossibility. In some cases fluids have been conveyed to the stomach in connection with some solid. The person thus afflicted appears to be in good health, but in five or six years death ensues from actual starvation. The suffering of such a one was described to me as most horrible."

—*Cystitis treated by Benzoic Acid.*—In a very marked case of severe inflammation of the bladder, (cystitis,) attended with very offensive ammoniacal urine, in one of Mr. Lloyd's wards at St. Bartholomew's, we were particularly struck recently by the improvement produced by the use of benzoic acid, or rather tinct. benzoin co. The former medicine is easily given in mucilage of linseed, and has been found very valuable in checking the ammoniacal tendency of the urine in enlarged prostates. Decoction of uva ursi and hops is also a useful combination; or decoction of pareira brava, with tincture of buchu. Mr. Lloyd has had a very large experience in these cases, as in his earlier days he used to assist Mr. Abernethy in passing the catheter for some of his patients, when that celebrated surgeon was unable to attend. The benzoic acid he believes to be a decided advantage.

—St. Luke's will, we learn, be partially opened this spring, under the medical direction of Dr. Eigenbrodt as resident physician.

—*Erratum.*—The proof of Prof. Frick's article was delayed so long by accidents and the post office, that the article was sent to press without his corrections. Our readers will please make these corrections: On page 247, line 29, read *Parkes*, instead of *Parker*; and in line 30, read *elimination*, instead of *diminution*. In the last line of p. 248, read *Garrod* for *Gornod*. Page 250, line 2, read *non* for *man's*. Page 253, *relation* of, for *relative*.